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I. OUR SIX-FOOTED RIVALS.

LET us suppose that, having no previous acquaintance with the subject, we were suddenly informed, on good authority, that there existed in some part of the globe a race of beings who lived in domed habitations, aggregated together so as to form vast and populous cities, —that they exercised jurisdiction over the adjoining territory, laid out regular roads, executed tunnels underneath the beds of rivers, stationed guards at the entrance of their towns, carefully removed any offensive matter, maintained a rural police, organised extensive hunting expeditions, at times even waged war upon neighbouring communities, took prisoners and reduced them to a state of slavery,—that they not merely stored up provisions with due care, to avoid their decomposition by damp and fermentation, but that they kept cattle, and in some cases even cultivated the soil and gathered in the harvest. We should unquestionably regard these creatures as human beings who had made no small progress in civilisation, and should ascribe their actions to reason. If we were then told that they were not men, and they were in some places formidable enemies to man, and had even by their continued molestations caused certain villages to be forsaken by all human occupants, our interest would perhaps be mixed with some little shade of anxiety lest we were here confronted by a race who, under certain eventualities, might contest our claim to the sovereignty of the globe. But when we learn that these wonderful creatures are insects some few lines in length our curiosity is cooled; we are apt, if duly guided by dominant prepossessions, to declare that the social organisation of these beings is not civilisation, but at most *quasi*-civilisation,—that their guiding principle is not reason, but "instinct,"

or *quasi*-intelligence, or some other of those unmeaning words which are so useful when we wish to shut our eyes to the truth. Yet that ants are really, for good or evil, a power in the earth, and that they seriously interfere with the cultivation and development of some of the most productive regions known, is an established fact. A creature that can lay waste the crops of a province or sack the warehouses of a town has claims upon the notice of the merchant, the political economist, and the statesman, as well as of the naturalist.

Many observers have been struck with the curious mixture of analogies and contrasts presented by the Annulosa and the Vertebrata. These two classes form, beyond any doubt, the two leading subdivisions of the animal kingdom. To them nineteen-twentieths of the population of the dry land, both as regards individuals and species, will be found to belong, and even in the world of waters they are largely represented. At the head of the Vertebrata stands the order of the Primates, culminating in man. At the head of the Annulosa the corresponding place is taken by the Hymenopterous insects. It is very remarkable—as first pointed out, we believe, by Mr. Darwin—that these two groups of animals made their appearance on the earth simultaneously. But along with this analogy we find a contrast. Man stands alone among the Primates as a socially organised being, possessing a civilisation. Among the Hymenoptera the lead is undoubtedly taken by the ants, which, like man, have a brain much more highly developed than that of the neighbouring inferior groups. But there is no one species of ant which enjoys a pre-eminence over its congeners anything at all approaching in its nature and extent to man's superiority over the gorilla or the mias. What may be the cause of this contrast we know not. Perhaps it is merely due to the tendency of the Annulosa to branch out into a scarcely numerable host of forms, whilst the vertebrate structure, less plastic, lends itself more sparingly to variation. Perhaps, on the other hand, lower human or higher ape-forms than any now existing have been extirpated, as the traditions of many ancient nations would seem to admit.

At any rate, whilst the superiority of the ants as a group to the remaining Hymenoptera, to all other insects, and to the rest of the annulose "sub-kingdom" is undisputed, we are unable to decide which species of ant is elevated above the rest of the Formicide family. Possibly more extended and more systematic observations may settle this interesting question. According to our present knowledge the claims

of the agricultural ant, of Western Texas (*Myrmica barbata*), seem perhaps the strongest. This species, which has been carefully studied by Dr. Lyncecum, for the space of twelve years, is, save man, the only creature which does not depend for its sustenance on the products of the chase or the spontaneous fruits of the earth. As soon as a colony of these ants has become sufficiently numerous they clear a tract of ground, some 4 or 5 feet in width, around their city. In this plot all existing plants are eradicated, all stones and rubbish removed, and a peculiar species of grass is sown, the seeds of which resemble very minute grains of rice. The field—for so we must call it—is carefully tended by the ants, kept free from weeds, and guarded against marauding insects. When mature, the crop is reaped and the seeds are carried into the nest. If they are found to be too damp they are carefully carried out, laid in the sunshine till sufficiently dry, and then housed again. This formation of a plot of cleared land—or, as Dr. Lyncecum not very happily terms it, a pavement, is a critical point in the career of a young community. Any older and larger city which may lie within some fifty or sixty paces looks upon the step as a *casus belli*, and at once marches its armies to the attack. After a combat, which may be prolonged for days, Providence declares in favour of the largest battalions, and the less numerous community is exterminated, fighting literally to the last ant. Where a colony is unmolested it increases rapidly in population, and undertakes to lay out roads: one of these, from 2 to 3 inches in width, has been traced to a distance of 100 yards from the city. These ants are not very carnivorous, nor do they damage the crops of neighbouring farmers. Persons who intrude upon the "pavement" are bitten with great zeal, but otherwise the species may be regarded as harmless. One creature alone they seem to tolerate on their "pavement,"—the so-called small black "erratic" ant,—which, as Dr. Lyncecum conjectures, may be of some use to them, and which is therefore allowed to build its small cities in their immediate neighbourhood. If it becomes too numerous, however, it is got rid of, not by open war, but by a course of systematic and yet apparently unintentional annoyance. The agricultural ants suddenly find that it is necessary to raise their pavement and enlarge the base of their city. In carrying out these alterations they literally bury the nests of their neighbours under heaps of the small pellets of soil thrown up by the prairie earthworms, and continue this process till the erratic ants in sheer despair remove to a quieter spot.

Concerning the government either of the agricultural ants or of other species our knowledge is of a very negative character. The queens, or rather mothers, of the city are indeed treated with great attention, but their number is quite indefinite, and, unlike female hive-bees, no jealousy exists between them. How their migrations, their wars, their slave-hunts are decided on, or even how the guards on duty are appointed, and the visiting parties selected who go round to inspect the works, and who sometimes insist on the destruction and rebuilding of any badly-executed portion, we are utterly ignorant. The outer manifestations of ant-life we have to some extent traced, but its inner springs, its directing and controlling powers have eluded our observation.

It has been remarked in the "Quarterly Journal of Science" that ants, unlike man, have solved the problem of the practical organisation of communism: this is literally true. In a formicary we can detect no trace of private property; the territory, the buildings, the stores, the booty, exist equally for the benefit of all. Every ant has its wants supplied, and each in return is prepared to work or to fight for the community as zealously as if the benefit of such toil and peril were to accrue to itself alone. If the principle —so common among men—that there is no harm in robbing or defrauding a municipal body, or the nation at large, crops up in an ant-hill at all, it must evidently be stamped out with an old-fashioned promptitude. But to understand why the ant has succeeded where man has failed, we must turn to certain fundamental distinctions between human and ant society, or perhaps, speaking more general, between the associations of vertebrate and those of annulose animals. A human tribe or nation—and in like manner, *e.g.*, a community of beavers or of rooks—is formed by the aggregation not of single individuals, but of groups, each consisting of a male, a female, and their offspring. The social unit among vertebrates, therefore, is the family, whether permanent or temporary, and whether monogamous or polygamous. In numberless cases the family exists without combining with other families to form a nation, but we greatly doubt if there exists a single case of a vertebrate nation not formed of and resolvable into families.

Among the Annulosa this is reversed. The family among them scarcely exists at all. Rarely is the union of the male and the female extended beyond the actual intercourse, all provision for the future young devolving upon the latter alone. Among the rare exceptions to this rule we may

mention the burying-beetle and some of the dung-beetles, both sexes of whom labour conjointly to find and inter the food in which the eggs are to be deposited. Generally speaking, moreover, the young insect never knows—never even sees—its parents, who in most cases have died before it has emerged from the egg. Among non-social insects the earwig and a few other Orthoptera form the chief exceptions. Where a regularly organised society, a nation, or tribe exists among annulose animals, it is not formed by the coalescence of families to a higher unity. The family, if it can be said to exist at all, is conterminous and identical with the nation. This absence of a something whose claims are felt by all ordinary men to be stronger than those of the State has rendered the successful organisation of the "Commune" feasible among ants, and among other social Hymenoptera, such as bees, wasps, &c. With them the State has no rival, and absorbs all the energies which in human society the individual devotes to the interests of his family. We thus see that theorists on social reform have been, from their own point of view, logically consistent in attacking the institution of marriage and the whole system of domestic life: they have sought to abolish the great impediment to the Commune, and to approximate man to the condition of our six-footed rivals, and to constitute society not as heretofore of molecules, but of atoms.

But it is not enough to show that the failure of Communism among mankind and its success among certain Hymenopterous insects are due to the existence and the power of the family in the former case, and to its absence in the latter. We have yet to enquire into the wherefore of so important a distinction. Vertebrate society, where it exists at all, is founded on family life, because every vertebrate animal is sexual, and as such is attracted to some individual of the opposite sex by the strongest instinct of its nature, that of self-preservation alone excepted. Invertebrate society, where it exists in perfection, as among the Hymenoptera, is not formed by a union of families, because the great majority of Hymenopterous individuals (in the social species) are non-sexual, neuter, incapable of any private or domestic attachments, and devoted to the community alone. To attempt, without the existence of such an order, to introduce the social arrangements of the ant —*i.e.*, Communism—among mankind is as futile and as irrational as the endeavour to fly without wings: the very primary conditions for success are wanting.

It may not be amiss to examine a little further in the

same direction. Among men there is a great diversity both in intellect and in energy. The more highly-endowed individual, if he does not leave his children in a better position, materially speaking, is likely to transmit to them his own personal superiority. In this manner the theoretical equality assumed as one of the bases of Communism is in practice annihilated. Among ants nothing of this kind can prevail. The workers and the fighters are sexless. If any individual is superior to its fellows in strength or in intelligence—and we have every reason to believe that such must be the case—it has no posterity to whom its acquisitions could be bequeathed or its personal superiority handed down. Hence the formation of an aristocracy is impossible, and whatever benefit may result from the labours of such an exceptional individual flows to the entire community. In the converse manner the formation of a pariah-, a criminal-, or a pauper-class is frustrated, and the public is not burdened with useless or dangerous existences.

It is indisputable that this arrangement, joined to the brief term of insect-life, must greatly retard the progress of the ant in civilisation. It has been remarked that were human life longer our development in knowledge and in the arts would be much more rapid. Take our present condition: by the time a man has completed his education, general and special,—has fully developed his own mental faculties and mastered the position of the subject he has selected,—he will be rarely less than five and twenty years of age. By the time he is fifty, as a rule, his power of origination begins to decline, and the remainder of his life is spent more in completing and rounding off the work of his younger days than in making fresh inroads into the unknown. Did our full vigour of intellect extend over a century, instead of over a fourth of that duration, we should undoubtedly effect much more. On the other hand, a shortening of our time of activity would have a powerfully retarding effect on the career of discovery and invention. Can we then wonder if the short-lived ant and bee sometimes appear to us stationary in their civilisation? But this very brevity of the career of each individual acts decidedly in favour of the preservation of social equality. If either ant or man is disposed to rise or to fall, then the shorter the time during which such rise or fall is possible the better will the uniform level of society be preserved. To prevent misunderstanding we must remark that castes with a corresponding difference of duties, and, according to some authorities, with a diversity of honour also, do occur in the

ant-hill; but within each caste all are on an exactly equal footing.

If we compare the zoological rank of our "six-footed rivals" with our own, we must, from one point of view, concede them a higher position. The more perfectly developed is any animal the more do we find it possessed of an especial organ for the discharge of every function. In like manner it may be contended that, as a species rises in the scale of being, duties once indiscriminately performed by all the species are assigned to distinct individuals. Among the humbler groups of the animal kingdom the whole reproductive task is performed by all members of the species. In other words, hermaphroditism prevails. As we ascend to higher groups the sexes are separated, and the species becomes dimorphous. This arrangement prevails among all vertebrate animals, and among a large majority of annulose species. We find here already, however, one of those contrasts which so often prevail between these two great series of beings. Among vertebrates, and especially in mankind, the function of the female sex seems limited to the nurture —intra- and extra-uterine—of the young. Were man immortal and non-reproductive, woman's *raison d'etre* would disappear. Among Annulosa the very reverse holds good; the females are as a rule larger, stronger, and more long-lived, whilst the task of the male seems limited to the fecundation of the ova. This being once performed, his part is played. Among butterflies, moths, and ants his death speedily follows, whilst among spiders he is generally killed and devoured by his better-half. This predominance of the female sex seems to prepare the way for the phenomenon which we recognise among the social Hymenoptera. Here the species become no longer dimorphous, but polymorphous. In other words, in addition to the males and females, whose task is now exclusively confined to the mere function of reproduction, there are, as we have seen, one or more forms of females, sexually abortive, but so developed in other respects as to form the castes of workers and fighters, upon whom the real government of the ant-hill belongs, who provide for its enlargement, well-being, and defence.

It may, we think, be legitimately contended that the development of a distinct working order is a step in advance similar to that taken by the distribution of the sexual functions among two different individuals—that the polymorphic species is higher than the dimorphic, just as the dimorphic is higher than the monomorphic.

Of the development of a neuter order among vertebrate animals, and especially among mankind, we know nothing which can be fairly called a trace. But in comparing the two civilisations, that of man and that of the ant, we must be struck with the fact that the former has from time to time imitated this peculiar feature. The attempts, however, whether made by the devotion of certain classes to celibacy or by actual emasculation, have been as unsuccessful as the sham elephants of Semiramis. Celibates retaining the sexual appetite, but deprived of its legitimate exercise, have always been a disturbing force in society. On the other hand, emasculation, instead of—as might have been perhaps, *a priori*, anticipated—increasing the powers of body and mind, enfeebles both. What would be the moral and social effects of the appearance of a neutral form of the human species analogous to the working-bee or ant it is impossible to foresee; but we may venture to surmise that they would not be entirely desirable.*

It may be suggested that the institution of caste among so many human races is an adumbration of the natural castes existing among social insects, each devoted to some especial function.

The remarkable intelligence of ants has from very early ages made a profound impression on man. Cicero considered them possessed of "mind, reason, and memory."† To the present day those who watch the formicary, not in order to defend prepossessions, but to arrive at truth, come to the same conclusion, unpopular though it may be. We sometimes wonder whether ants, like men, consider themselves the sole reasonable beings on the globe, prove their position by sound *a priori* arguments, and accuse those who take a different view of "scepticism" or "agnosticism."

When it is no longer possible to meet with a flat denial all instances of correct inferences drawn and of happy contrivances adopted by brutes in general and by ants in particular, the writers who still claim reason as the exclusive prerogative of man bring forward a curious objection: they urge that we should likewise collect proofs of animal folly and stupidity, and seem to think that these latter instances would nullify any conclusion that might be drawn from the former. That instances are numerous where some animal

* It is very remarkable that among the Termites, which though improperly called "white ants" belong to a different order of insects, neuters exist. These, however, do not appear to be imperfectly developed females. It would thus seem that among insects social organisation necessitates a class of sexless individuals.

† "Mens, ratio, et memoria."

fails to draw an inference—very obvious, in our view—or to adopt some very simple expedient we do not deny, and that their conduct hence seems strangely chequered we admit. What, *e.g.*, can seem more inconsistent than the following cases? Sir John Lubbock, to test the intelligence of ants, placed a strip of paper so as to serve as a bridge or ladder for some ants which were carrying their pupæ by a very roundabout away. The slip was, however, purposely left short of its destination by some small fraction of an inch. It would have been very easy for the ants either to have dropped themselves and their burden down this short distance, or to have handed the pupæ to the other ants below, or to have piled up a small amount of earth from below so as to meet the slip of paper, and thus make the descending road continuous. They adopted, however, none of these expedients, but continued to travel the roundabout way.

On the other hand, Mr. Tennant tells us that *Formica smaragdina*, in forming its dwellings by cementing together the leaves of growing trees, adopts the following method:—A line of ants, standing along the edge of one leaf seize hold of another, and bring its margin in contact with the one on which they are posted. They then hold both together with their mandibles, whilst their companions glue them fast with a kind of adhesive paper which they prepare. If the two leaves are so far apart that a single ant cannot reach from one to another, they form chains with their bodies to span over the gap. The same author also informs us that certain Ceylonese ants, when carrying sand or dry earth for the construction of their nests, glue several grains together so as to form a lump as large as they can carry, and thus economise time and labour.

Mr. Belt, in his "Naturalist in Nicaragua" (p. 27), gives the following account of the manner in which the *Ecitons*, or foraging ants of Central and South America, deal with what may be called engineering difficulties:—"I once saw a wide column trying to pass along a crumbling, nearly perpendicular slope. They would have got very slowly over it, and many of them would have fallen, but a number having secured their hold and reaching to each other remained stationary, and over them the main column passed. Another time they were crossing a water-course along a small branch, not thicker than a goose-quill. They widened this natural bridge to three times its width, by a number of ants clinging to it and to each other on each side, over which the column passed three or four deep; whereas, except for this expedient, they would have had to pass

over in single file, and treble the time would have been consumed."

Again, *Eciton legionis*, according to Mr. Bates, when digging mines to get at another species of ant whose nests they were attacking, the workers are divided into parties, "one set excavating and another set carrying away the grains of earth. When the shafts became rather deep the mining parties had to climb up the sides each time they wished to cast out a pellet of earth, but their work was lightened for them by comrades who stationed themselves at the mouth of the shaft and relieved them of their burdens, carrying the particles with an appearance of foresight which quite staggered me, a sufficient distance from the edge of the hole to prevent it from rolling in again."

What, then, are we to learn from these somewhat inconsistent cases? Are we to conclude that Sir John Lubbock, Mr. Belt, Mr. Bates, and Mr. Tennant must be careless and incompetent observers? Assuredly not. Are we to believe that ants are stupid, irrational creatures, and that when they do anything right it must be regarded as an accident or ascribed to that convenient phantom, instinct? Still less: the well-established cases which are on record agree badly with either of these suppositions. The true explanation of the difficulty is that, like all finite intelligences, ants are not equally wise on all occasions. Sometimes they hit upon the best expedient for evading or overcoming an obstacle, but sometimes, under circumstances not more complicated, they fail. This is doubtless the case with man himself. If contemplated by some being endowed with higher reasoning powers, would he not be pronounced a most curiously inconsistent mixture of sagacity and stupidity, now solving problems of no small difficulty, and now standing helpless in presence of others even more simple? That such is in reality the case with man is proved by the history of discoveries, and of their reception. Do we not always say when we hear of any great step, whether in scientific theory or in the practical arts, "How simple, how natural!" Yet, simple and natural as it is, all sorts and conditions of men lived for centuries without opening their eyes to it. To those who, on the score of incidental blunders and stupidities, deny the rationality of animals, we would hold up the ever-memorable "egg" of Columbus, and exclaim "What, gentlemen, do you expect the ant to be more uniformly and consistently intelligent than your erudite selves?"

Concerning the language of ants no small diversity of opinion has prevailed; but among actual observers the

general conclusion is that these tiny creatures can impart to each other information of a very definite character, and not merely general signals, such as those of alarm. It has been found that ants fetched by a messenger for some especial purpose seem, when they arrive at the spot, to have some knowledge of the task which is awaiting them, and set about it at once without any preliminary investigation. The cases which we quote elsewhere from Mr. Belt are very conclusive on this point. In order to decide whether ants are really fetched to assist in tasks beyond the strength of any one of their number, Sir John Lubbock instituted a very interesting and decisive experiment. It is well known that if the larvæ of ants are taken out of the nest, the workers never rest till they have fetched them back. Sir John Lubbock took a number of larvæ out of his experimental formicary, and placed them aside in two parcels very unequal in number. Each of these lots was soon discovered by an ant, who at once fell to work to carry the larvæ back to the nest, and was soon joined by others, eager to assist. The observer reasoned thus:—If these ants have come to the spot by accident, it is probable that the number who arrive at each lot will be approximately equal. On the other hand, if they are intentionally fetched to assist in removing the larvæ, the number in each case will most likely bear some proportion to the amount of work to be done. The result was that the large heap of larvæ was visited by about three times as many ants as the small one. Hence the inference is plain that ants can call assistance to any task in which they are engaged, that they can form some estimate of the amount of labour that will be required, and can make their views in some manner known to their companions. The manner in which, when on the march, they are directed by their officers, and the promptitude and precision with which a column is sent out to seize any booty indicated by scouting parties, show likewise a completeness and precision of language very different from anything we observe in quadrupeds and birds.

But as to the nature of this language, which Mr. Belt rightly calls "wonderful," we are as yet very much in the dark. Sounds audible to our ears they scarcely can be said to emit. Their principal organs of speech are doubtless the antennæ: with these, when seeking to communicate intelligence they touch each other in a variety of ways. There can be no doubt that, with organs so flexible and so sensitive, an interchange not merely of emotions but of ideas must be easy.

But there is another channel of communication which deserves to be carefully investigated. We know that the language of vertebrates, or at least of their higher sections, turns on the production or recognition of sounds. What if the language of social insects should be found to depend, in part at least, on the production and recognition of odours? We have already full proof that their sense of smell is developed to a degree of acuteness and delicacy which utterly passes our conceptions of possibility, and to which the scent of the keenest hound presents but a very faint approximation. Collectors of Lepidoptera are well aware that if a virgin female moth of certain species is enclosed in a box, males of the same species will make their appearance from distances which may be relatively pronounced prodigious. As soon, however, as the decoy has been fecundated this attraction ceases. This is only one among the many phenomena which testify to the wonderful olfactory powers of insects. So much, then, for the recognition of odours. Nor is their production among insects a matter open to doubt. Scents, distinctly perceptible even to our duller organs, are given off by many. The pleasant odour of the musk-beetle, and the offensive smells of the ladybirds, the common ground-beetles, the oil-beetles, the Spanish fly, and the "devil's coach-horse"—hence technically named *Gœrius olens*—are known to every tyro in entomology. The next question is, Are these odours at all under the control of the insect, and capable of being produced, suppressed, or modified at will? We have noticed many instances where the odours of insects became more intense under the influence of anger or alarm. A peculiarly pungent odour is said to issue from a bee-hive if the inmates are becoming excited.

The possibility of a scent-language among insects must therefore be conceded. Mr. Belt thinks that the *Ecitons* mark out a track which is to be followed by their comrades by imparting to it some peculiar odour. He says:—"At one point I noticed a sort of assembly of about a dozen individuals that appeared in consultation. Suddenly one ant left the conclave, and ran with great speed up the perpendicular face of the cutting without stopping. It was followed by others, which, however, did not keep straight on like the first, but ran a short way, then returned, and then again followed a little farther than the first time. They were evidently scenting the trail of the pioneer, and making it permanently recognisable. These ants followed the exact line taken by the first one, though it was far out of sight. Wherever it had made a slight *détour*, they did

so likewise. I scraped with my knife a small portion of the clay on the trail, and the ants were completely at fault for a time which way to go. Those ascending and those descending stopped at the scraped portion, and made short circuits until they hit the scented trail again, when all their hesitation vanished, and they ran up and down it with the greatest confidence."

That among groups like the *Ecitons*, in which the sense of sight is imperfect or even totally wanting, enhanced delicacy of scent and touch must be required in compensation may be taken as self-evident. With the language of ants, and especially with a possible scent-language, is connected the faculty by means of which denizens of the same city recognise each other under circumstances of great difficulty. In the battles which take place between two nations of the same species, how, save by scent, do the tiny warriors distinguish friend from foe? We are told by some older observers that if an ant is taken from the nest, and restored after the lapse of several months, it is at once received by its companions and caressed, whilst a stranger ant introduced at the same time is rejected, and generally killed. To a great extent this has been confirmed by recent investigators. The returned exile was not indeed caressed, but was quietly allowed to enter the nest, whilst a stranger was at once greeted with hostile demonstrations. It has been maintained that this power of recognition is destroyed by water, and that ants will treat a comrade as an enemy if he has received a drenching. This, however, is evidently a mistake. To prevent rain from penetrating into the nests of the agricultural ant the guards block up the doorways with their bodies, and are often drowned at their posts. But their companions are not thereby prevented from recognising them, as they try to bring the dead bodies to life.

Even more wonderful than the mere intelligence of the ant is its power of organisation—the point, probably, in which it approaches most closely to man. Suppose that ants, instead of forming nations, lived like most creatures, merely in pairs, each endeavouring to rear a young brood, who when mature would enter upon a similarly isolated career. Let them be as brave, as intelligent, and as strong as they now are, still how humble and insecure would be their position! Against the attacks of the giant spiders, centipedes, hornets, and wasps of warm climates they could make no effectual resistance. Prey which in their present condition they easily secure would escape them, or would

scarcely even notice their puny efforts. In short, there is every reason to believe that many of their species would become extinct, and that the remainder would live, so to speak, mainly on sufferance, playing no appreciable part in the economy of the globe. Turning from this hypothetical survey of the ant as an individual, unorganised being, to its actual condition, we see the most striking contrast. Mr. Belt gives the following graphic account of the excitement caused by a marching column of *Ecitons* in the primeval forests of Nicaragua:—"My attention was generally first called to them by the twittering of some small birds belonging to different species. On approaching, a dense body of the ants, 3 or 4 yards wide and so numerous as to blacken the ground, would be seen moving rapidly in one direction, examining every cranny and underneath every fallen leaf. On the flanks and in advance of the main body smaller columns would be pushed out. These smaller columns would generally first flush the cockroaches, grasshoppers, and spiders. The pursued insects would rapidly make off, but many, in their confusion and terror, would bound right into the midst of the main body of ants. At first the grasshopper, when it found itself in the midst of its enemies, would give vigorous leaps, with perhaps two or three of the ants clinging to its legs. Then it would stop a moment to rest, and that moment would be fatal, for the tiny foes would swarm over the prey, and after a few more ineffectual struggles it would succumb to its fate and soon be bitten to pieces and carried off to the rear. The greatest catch of the ants was, however, when they got amongst some fallen brushwood. The cockroaches, spiders, and other insects, instead of running right away, would ascend the fallen branches and remain there whilst the host of ants were occupying all the ground beneath. By-and-bye up would come some of the ants, following every branch and driving before them their prey to the ends of the small twigs, where nothing remained for them but to leap, and they would alight in the very throng of their foes with the result of being certainly caught and pulled to pieces.

"The moving columns of *Ecitons* are composed almost entirely of workers of different sizes, but at intervals of 2 or 3 yards there are larger and lighter-coloured individuals that often stop and sometimes run a little backward, stopping and touching some of the ants with their antennæ. They look like officers giving orders and directing the march of the column.

"The ants send off exploring parties up the trees, which

hunt for nests of wasps, bees, and probably birds. If they find any they soon communicate the intelligence to the army below, and a column is sent up immediately to take possession of the prize. I have seen them pulling out the larvæ and pupæ from the cells of a large wasps' nest, whilst the wasps hovered about, powerless, before the multitude of the invaders, to render any protection to their young."

Still more formidable are the "driver ants" of Tropical Africa, so called because on their approach even the lion, the elephant, and the huge python at once betake themselves to flight.

Nor are the purely vegetarian ants of less importance in the economy of the countries they inhabit. They decide, in a manner, what trees shall grow and what shall be exterminated, and it is only such as are comparatively distasteful to them that escape. In Nicaragua they render the acclimatisation of any foreign tree or vegetable a task of great difficulty. Mr. Belt was often told, on asking the reason why no fruit-trees were grown at certain places, "It is of no use planting them; the ants eat them up." These ants climb up the trees, when "each one, stationing itself on the edge of a leaf, commences to make a circular cut from the edge with its scissor-like jaws, its hinder feet being the centre on which it turns. When the piece is nearly cut off it is still stationed upon it, and it looks as though it would fall to the ground with it; but on being finally detached the ant is generally found to have hold of the leaf with one foot, and soon righting itself, and arranging its burden to its satisfaction, it sets off at once on its return."

An observer standing near the ant-hills "sees from every point of the compass ant-paths leading to them, all thronged with the busy workers carrying their leafy burdens. As far as the eye can distinguish their tiny forms, troops upon troops of leaves are moving up towards the central point and disappearing down the numerous tunnelled passages. The ceaseless toiling hosts impress one with their power, and one asks—What forests can stand before such invaders?" Concerning the use to which the ant-leaves are put some difference of opinion prevails; that they do not directly serve as food is admitted. Mr. Bates, from observations made in Brazil, concludes that "the leaves are used to thatch the domes which cover the entrances to their subterranean dwellings, thereby protecting from the deluging rains the young brood in the nests beneath." Mr. Belt, who has carefully examined the habits of an allied species in Nicaragua, believes that the real use they make of them

is as a manure, on which grows a minute species of fungus on which they feed,—that they are, in reality, mushroom growers and eaters. The reasons for this view are given in detail in Mr. Belt's work, and appear very satisfactory. But Mr. Bates's view may be correct also. In short, save man alone, there is no creature which can effect such widespread and profound alterations in the condition of a country as the tiny ant. It has been indeed mentioned in the "Quarterly Journal of Science" that the pig, the goat, and the rabbit have succeeded in extirpating the natural flora, and consequently to a great extent the fauna, of certain islands, such as St. Helena. Yet this takes place only in countries where there are no carnivorous beasts, birds, and reptiles to keep them in check. But in every warm and fruitful climate the ant is king. This power we perceive is not due to mere numbers; it is in great part the result of organisation. Other species of insects are perhaps even more numerous, and, individually considered, as capable of destructive action; but locusts, potato-beetles, mosquitoes, noisome as they may be considered, are, in comparison with ants, what a promiscuous mob is in comparison with a well-trained and organised army. Each ant, like an experienced soldier, knows—whether rationally or instinctively it matters not—that it will be systematically supported by its comrades. What would be the prospects of agriculture in Western Asia, in Northern Africa, or in the Western States of the American Union, if the locusts when engaged in desolating a field were to attack, *en masse*, any man or bird who should interfere with them? But, on the contrary, they allow themselves to be slaughtered in detail, each indifferent to the fate of his neighbour.

Ants evince that close mutual sympathy which to an equal extent can be traced probably in man alone, and which has in both these cases proved one of the primary factors in the development of civilisation. Had man been devoid of this impulse he would have remained a mere wandering savage—perhaps a mere anthropoid, occurring as a rare species in equatorial districts. Without a similar impulse the *Ecitons* would have ranked among the many solitary species of Hymenoptera. Of the mutual helpfulness of these same *Ecitons* Mr. Belt gives us some most interesting cases which came under his own observation:—"One day when watching a small column of these ants (*Eciton hamata*) I placed a little stone on one of them to secure it. The next that approached, as soon as it discovered its situation, ran backwards in an agitated manner, and soon

communicated the intelligence to the others. They rushed to the rescue: some bit at the stone, and tried to move it; others seized the prisoner by the legs, and tugged with such force that I thought the legs would be pulled off—but they persevered until they got the captive free. I next covered one up with a piece of clay, leaving only the ends of the antennæ projecting. It was soon discovered by its fellows, which set to work immediately, and by biting off pieces of the clay soon liberated it. Another time I found a very few of them passing along at intervals: I confined one of these under a little piece of clay, with his head projecting. Several ants passed it, but at last one discovered it and tried to pull it up, but could not. It immediately set off at a great rate, and I thought it had deserted its comrade, but it had only gone for assistance, for in a short time about a dozen ants came hurrying up, evidently fully informed of the circumstances of the case, for they made directly for their imprisoned comrade, and soon set him free. The excitement and ardour with which they carried on their exertions for the rescue could not have been greater if they had been human beings."

Such cases as these are of the greater moment because many other social and semi-social animals treat an unfortunate companion in a very different manner. It is on record that a rook, which had got entangled among the twigs of a tree, was pecked and buffeted to death by its neighbours, despite the efforts of its mate for its protection.

Facts are not wanting which show that the social organisation of ants takes cognizance of sanitary matters. In Australia they have been known to bury their dead, not without some degree of formality* according to their caste. In experimental formicaries in this country ants have been observed to throw the bodies of their dead companions into the water surrounding their dwellings. In the nests of almost all species great care is taken to preserve cleanliness. The agricultural ant of Texas removes any offensive matter placed near its city, and will even take the trouble to carry away the droppings of cattle that have fallen on its cleared ground. Any dung-rolling beetle which brings its ball of ordure within these sacred precincts is at once attacked and put to death, and the nuisance is quickly cut to pieces and carried to a distance.

Nor are laws on other matters wanting. Ants who have, from some unknown cause, refused to work have been

* Journal of Linnean Society, vol. v., p. 217.

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observed to be put to death. Among the agricultural ants prisoners have been known to be brought in by a fellow-citizen and handed over in a very rough manner to the guards who are always on duty on the level ground before the city, and who carry off the offender into the underground passages. What is his after fate is not known. It is almost needless to point out that even the faintest rudiment of law proves the existence of some notions of right and wrong, as well as of a power of communication which must go into minute details.

We have now to deal with the great question whether the civilisation of ants, like that of man, has been gradually and slowly developed by the accumulation of experience, or whether—as the believers in the fixity of habits and instincts still contend—it is primordial, coexistent with the species in all the details which we now observe. Direct historical evidence is here yet more difficult to obtain than as concerns animal structure. We smile, with just reason, at the French *savants* of the Egyptian expedition who imagined that by the study of the animal-mummies there preserved they might gain some light on—or rather find some argument *against*—the mutation of species. At the same time we readily admit that could we find a complete series of skeletons, anatomical preparations, or even photographs of the best-known animals made at intervals of a century and extending backwards for say a hundred thousand years, the doctrine of evolution would be brought to a crucial test. But concerning the former habits and instincts of animals correct information is far more difficult to obtain. The "stone book" is silent or oracularly vague. Even if we had written documents left us by some naturalist of the Miocene ages—if we can suppose such a being to have existed—what security should we have for the accuracy and the completeness of his researches?

To meet this difficulty an attempt remarkable for its subtle ingenuity has been made by Prof. Heer. He points out that, according to the reckoning of the most discreet geologists, at least a thousand centuries must have elapsed since Britain was severed from the continent of Europe. For this long stretch of time, therefore, British animals must have been cut off from their representatives in France, Belgium, and Switzerland. If, then, the habits of a certain slave-holding ant (*Formica sanguinea*) in England are found identical, as he maintains, with the habits of the same species in Switzerland, there is a strong presumption that its economy has undergone no change for the last hundred

thousand years. To this argument we must reply that the isolation between British and Continental species of insects is by no means so complete as is here assumed. Winged ants travel very considerable distances, and, if our memory does not deceive us, have been met with out at sea. That a part of a swarm should be blown over from the French coast to England, or *vice versâ*, is by no means improbable. And it is well known that if a party of working ants fall in with an impregnated female of their own species, they immediately lead her to their nest and install her in a royal apartment. That there may have been within the last ten thousand—or even one thousand—years direct intercommunication of this kind between the slave-making ants of England and those of Switzerland seems to us fully more probable than the contrary supposition.

Again, we may ask whether the conditions under which ants would be respectively placed in Switzerland and in England are not so closely analogous that their social development must proceed on parallel lines? In both they would encounter nearly the same climate, the same food, and the same enemies. Surely, therefore, a close correspondence in habits is no decisive proof of their immobility. But, after all, is there such an absolute accord between the habits of the Swiss and of the British ants as the validity of Professor Heer's argument would require? Mr. Darwin thinks that in the nests of the British *Formica sanguinea* there is a relatively smaller proportion of slaves, which therefore play a less important part in the economy of the ant-hill. Messrs. Kirby and Spence record a fact which, isolated as it is, seems to us to overthrow altogether the hypothesis of absolute stationariness. Ants have been found, namely, to establish their nest in the interval between the double casing of a glass bee-hive. Now, as such bee-hives are artificial objects and of very recent origin, they cannot have come in the way of the ants for any great length of time. They offered, however, a certain advantage in the uniform temperature and the shelter which they supplied. This fact must have been recognised by some prying ant, and the discovery being communicated to its comrades was turned to practical account. Is not this case the exact parallel of a step in the development of human civilisation? And if, as we see, ants can in one case observe a phenomenon, reason on such observation, and work out their conclusions in their daily life, we can certainly see no grounds for supposing that such processes may not have occurred often. In the case of larger animals, where observation is easier,

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changes of habits in accordance with new facilities or new dangers have been distinctly recognised. There can be no necessity for us to quote the cases of alterations in the nidification of birds given by Mr. Wallace.* Recent American observations show that the habits of many birds, mammalia, and even fishes, have undergone a very decided alteration in settled districts as compared with less frequented regions. All species have become more wary and circumspect in their movements, and are decidedly more nocturnal. The birds build their nests on higher trees, or in the densest thickets. Any unusual object placed in a river alarms the fishes more than a similar object would have done some years ago and more than it does now in solitary parts of the country. A new danger is recognised, and precautions are taken accordingly.

On carefully examining the habits of ants we find that there exist among closely-allied species, and even in different colonies of one and the same species, gradations which to our mind supply powerful evidence that such habits cannot have been primordial. The slave-making propensity and the reliance placed upon slaves occur in several species, but not to the same degree. *Polyergus rufescens*, for instance, is absolutely dependent upon its slaves, and would without them perish from sheer incompetence to manage its own affairs further than by conducting slave-hunts. It is a military aristocracy, which can fight, but will rather die than work. *Formica sanguinea*, on the other hand, has much fewer slaves, and restricts them to a much narrower sphere of duties, being itself capable of working as well as of fighting. It is curious that the raids of slave-holding ants are confined to worker-pupæ of the species which it subjugates. No instance has reached us of ants carrying off male and female pupæ with a view to raising a stock of slaves in their own city, without the necessity of obtaining them by war. Surely the most rational way of accounting for this slave-making propensity is to suppose that, as in the human race, it is a gradual outcome of war. Ants, in the wars which they are known to wage against different species, as well as against their own, would take prisoners —an undeniable fact—with the original intention of killing and devouring them. Some few of these victims, escaping immediate slaughter, might, if of a docile and submissive disposition, be found useful, and might hence be allowed to live in servitude. Prisoners of fiercer and more indomitable

* Contributions to the Theory of Natural Selection, p. 227.

species, if taken at all, are no doubt killed. The query here naturally arises, What happens in the not infrequent wars between two cities of the same species? Are the prisoners slaughtered, or are they incorporated with the victorious nation?

No less variation may be traced in the habits of the cattle-keeping ants. Of the honey-secreting Aphides and *Cocci* that serve them as milch-kine, some have large herds, some small ones, whilst others have none at all, and if they encounter an *Aphis* straightway kill and eat it. Is it not more probable that the ants first sought Aphides, like other insects, for this very purpose, but gradually discovered a way to turn them to better account than that a flock of Aphides was, by some wonderful coincidence or interposition, placed within the reach of the first ant-hill.

It would therefore, in our opinion, be exceedingly imprudent to declare that ant-civilisation has not advanced, may not now be advancing, and may be destined to take yet further steps in the future, especially if large and fruitful portions of the globe are long allowed to remain in an uncultivated or semi-cultivated state. But such advances must necessarily be slow, as in all cases where there are no means of recording the experience of one generation for the benefit of the succeeding, and where what among mankind would be known as oral intercourse is limited by shortness of life. What direction these future advances may take it is as difficult to indicate as to foretell the discoveries and inventions to be made by man during the next century. But we may safely say that they will not consist in the introduction of tools or weapons, or machinery. Were man, in proportion to his size, about twenty times as strong as he is at present,—were he provided by Nature with a pair of forceps, playing laterally, and capable of being used for felling trees, for excavating the ground, or for cutting off the heads of his enemies, he would scarcely have been a tool-inventing and tool-using animal. A being which, like the Sauba ants of Brazil, can construct a tunnel underneath the bed of a river as wide as the Thames at London Bridge, is in no need of shovels, pickaxes, or barrows.

That ants, in tropical climates, occasion much loss and annoyance to man is indisputable; yet the annihilation of all kinds of ants, were such a measure practicable, would scarcely be prudent. Here, as elsewhere, the rule holds good that small Carnivora are to be cherished, and small Herbivora and Omnivora destroyed. The carnivorous ants, such as the *Ecitons*, are invaluable, from the myriads of

cockroaches, scorpions, centipedes, venomous spiders, grasshoppers, and even rats and mice that they destroy. They keep down serpents, also, by devouring their eggs. The plant-eaters, on the contrary, and especially the leaf-cutters, are an unalloyed evil, and their destruction ought to be attempted in a much more systematic way than what takes place at present. Nor can the "cattle-keeping" ants be tolerated. Even though they may not in their own persons attack the fruits and the leaves of useful trees, they compass injury to the latter by cherishing and defending swarms of such pernicious vermin as the Aphides of temperate regions and the scale-insects and tree-hoppers of warmer climates. All these live by sucking the juices of plants, and over them the ants watch with a wonderful care—defending them from the attacks of birds, wasps, ichneumons, and other creatures who would rid the poor plant of its parasites. They have even been known to build galleries of clay over the surface of a pine-apple, in order to shelter the *Cocci* who were destroying the fruit.

Mr. Belt found that a red passion-flower, which secretes honey from glands on its young leaves and on the sepals of its flower-buds, was carefully guarded by a certain species of ant (*Pheidole*), who consumed the honey, and who furiously drove off all leaf-cutters and other intruders. But after a couple of seasons a colony of parasitical scale-insects, which secrete honey, established themselves upon the passion-flower, to its great injury. The ants transferred their care and attention to these, and, from the guardians of the plant, became indirectly, but not the less substantially, its enemies. This is a striking proof of the untrustworthy character of our insect—or, more generally speaking, of our animal—allies. At one moment they may be defending our property from depredation, but on a slight change of circumstances their interests may cease to coincide with our own, and they may go over to our enemies. The question what animal-species we ought to protect and which to destroy, and how far we ought to go in each case, becomes on closer inspection exceedingly complicated.

As an example of an omnivorous ant we may take the "fire-ant" of the Amazon, of which Mr. Bates gives us a striking account*:—"Aveyros may be called the head-quarters of the fire-ant, which might be fittingly termed the scourge of this fine river. It is found only on sandy soils, in open places, and seems to thrive more in the neighbourhood

* Naturalist on the River Amazon.

of houses and weedy villages, such as Aveyros: it does not occur at all in the shades of the forest. Aveyros was deserted a few years before my visit on account of this little tormentor, and the inhabitants had only recently returned to their houses, thinking its numbers had decreased. It is a small species, of a shining reddish colour, not greatly differing from the common stinging ant of our own country (*Myrmica rubra*), except that the pain and irritation caused by its sting are much greater. The soil of the whole village is undermined by it; the ground is perforated with the entrances to their subterranean galleries, and a little sandy dome occurs here and there where the insects bring their young to receive warmth near the surface. The houses are overrun with them; they dispute every fragment of food with the inhabitants, and destroy clothing for the sake of the starch. All eatables are obliged to be suspended in baskets from the rafters, and the cords well soaked with copaiba balsam, which is the only means known of preventing them from climbing. They seem to attack persons out of sheer malice: if we stood for a few moments in the street, even at a distance from their nests, we were sure to be overrun with them and severely punished, for the moment an ant touched the flesh he secured himself with his jaws, doubled in his tail, and stung with all his might. When we were seated on chairs in the evenings, in front of the house, to enjoy a chat with our neighbours, we had stools to support our feet, the legs of which, as well as those of the chairs, were well anointed with the balsam. The cords of hammocks were obliged to be smeared in the same way to prevent the ants from paying sleepers a visit." The ravages of the leaf-cutting ant (*Oicodona*), or Saubas of the Brazilians, have been already mentioned. But it also invades houses and carries off articles of food on a far wider scale than is ever done by rats or mice. It is capable of carrying off such a quantity as two bushels of mandioca meal in the course of a single night. Unfortunately the Sauba has few enemies. The number of these depredators who fall a prey to birds, spiders, wasps, tiger-beetles, &c., is too small to be of any importance. The *Pseudomyrma bicolor* easily repels them if they come to clip the leaves of the bull's-horn acacia on which it resides, but it is not sufficiently numerous to pursue and destroy them. The *Ecitons* have never been known to storm the nests of the Sauba. Thus, as we often find, for the greatest mischiefs Nature provides no remedy, and man must step into the breach armed with carbolic acid and corrosive sublimate.

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II. THE ATMOSPHERE CONSIDERED IN ITS GEOLOGICAL RELATIONS.

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THE gaseous envelope which surrounds our globe plays a very considerable part in the chemical changes ever going on in rock formations, whether actually at the surface—as in what is called the "weathering" of rocks—or in the less apparent, but perhaps more powerful, action carried on at greater depths whither the atmospheric gases are conveyed by the action of percolating water. It has been shown, by the experiments of Prof. Rogers,* as well as by those of Bischof† and others, that perfectly pure water has a very appreciable solvent effect on rocks and minerals; and that power is immensely augmented, and capability to produce even more momentous alterations in the form of chemical decomposition added, when it is charged with carbonic acid, oxygen, nitric acid, and other matters derived directly or indirectly from the atmosphere.

While, on the one hand, the influence of the atmosphere disintegrates and destroys rock-masses, on the other it is mighty in building them up. Without the small percentage of carbonic acid contained in air—a quantity relatively minute, but in the aggregate enormous—there could be no vegetation. The vegetable kingdom, which obtains its supplies of carbon from those insignificant traces, would be wanting, and there could be none of the coal-beds which form such important members of our rock-formations. This is a direct and palpable case. But if we consider the immense masses of limestones which have been accumulated from those of the Laurentian period, and for aught we know before it, up to the coral reefs of the present day, and which must owe their being indirectly to carbonic acid of former atmospheres, we shall have some idea of the stupendous results attained by very small means, provided time enough be granted.

A drop of rain water absorbs a trace of carbonic acid from the atmosphere, falls on a rock containing lime in some

* Report Brit. Assoc., 1849. Trans. of Sections, p. 40.
† Elements of Chem. Geol., English ed., vol. i. pp. 57, 58.

form, dissolves the lime as bicarbonate, carries it down to the ocean, and finally gives it up to become part of the skeleton of a coral or mollusc, which in its turn may form a portion of an immense mass of limestone rock.

The atmosphere mainly consists of a mechanical mixture of oxygen and nitrogen; these, however, bear to each other an almost constant proportion, any variations being extremely minute. The composition by volume is found to be as follows:—

Oxygen	20·80
Nitrogen	79·20

Carbonic acid,* 3 vols to 10 vols. in 10,000 vols.
Ammonia, a trace; 0·1 to 135 vols. in 1,000,000.
Nitric and sulphuric acids, traces occasionally.

The respective amounts of oxygen and nitrogen do not vary to the extent of as much as 1 per cent, even in exceptional cases. Regnault's analyses of samples of air collected in various parts of the globe gave very close results, the percentage of oxygen being to all intents and purposes identical, viz., 20·9 per cent. Air collected by Sir James Ross in the Arctic Regions did not differ in this respect from that collected at Paris, or at Ecuador in South America; the very slight differences that have been observed not exceeding those noticed in air collected at the same place at different times: and the same results have been obtained from air collected at the summit of Mont Blanc, and even from that taken at a height of 21,000 feet by Gay-Lussac during a balloon ascent. There is therefore a marked uniformity in the aërial mixture under all circumstances.†

It has not yet been explained how it is that a mere mechanical mixture should have this constant composition, but it is certain that the gases are not chemically combined—

1. Because the proportion of the constituents bear no simple relation to the atomic or combining weights of those elements.
2. When they are mixed in the proper quantities there is no contraction, nor is there any evolution of heat, and the mixture acts in every way as air.

* Strictly carbonic anhydride; but I shall use the less scientific but more familiar term in this paper to designate it, in accordance with geological custom as regards this gas. Indeed in its geological relations it may be regarded as a true acid when dissolved in water.

† From some recent observations by Boussingault, and Miller, it would appear the amount of oxygen slightly differs at various heights. Mendeleeff thinks Gay-Lussac's results are probably incorrect (*Bull. Soc. Chim.* [2], xxv., 394). However, we have hardly decisive information yet on this point.

3. Water through which air is passed dissolves the two gases in very different proportions to those in which they are associated, the oxygen being very soluble, while the nitrogen is not taken up to any notable extent.

Carbonic Acid.

Although the bulk of the atmosphere is made up of the two gases just referred to, these do not take so active shares in geological matters as the almost infinitesimal trace of carbonic acid present. This, then, deserves the place of honour in the following pages, and it will be seen that there is a great deal to be said about it. We shall therefore defer the consideration of the behaviour of the other constituents for a little while.

The amount of carbonic acid ranges from about 3 to 10 volumes in 10,000 volumes of air, and the proportion varies between these limits in different localities, owing to many modifying causes. In the neighbourhood of towns or cities it will be much increased by the combustion of fuel, the exhalations of animal life, and the decay of organic matters. In the vicinity of large forests, swamps, and fens, vegetable decay will also augment it, though at the same time the living vegetation there will help to re-absorb it, or, to speak exactly, to decompose it. Near volcanoes the air will be more or less impregnated with it; and from many mineral springs, and subterranean caves and fissures, a very considerable quantity of this gas is discharged into the atmosphere. The percentage of carbonic acid also varies slightly between day and night.

Geological Effects.

So small a trace as even 10 in 10,000—taking the maximum, only 0·1 per cent—certainly does not at first sight seem capable of performing any very great geological work; but we must recollect that the vast quantities of existing vegetation are entirely dependent on the carbon they obtain from the atmosphere, and the decay of vegetation, and consequent liberation of carbonic acid, has a very powerful effect in the alteration or solution of rocks. However, the direct action of atmospheric carbonic acid on rocks—both as a destructive and as a recuperative agent—must be anything but small, even at the present day. As to the latter, it is only necessary to refer to the immense coral reefs now being formed, while the widespread deposits of ooze and mud over the floors of the Atlantic and Pacific are largely

due to carbonic acid entrapped by rain-water and carried down into the ocean. On the one hand, the carbonate of lime previously conveyed by river waters is held in solution, and kept in a fit state for assimilation by marine organisms. On the other, the dead shells while sinking through great depths are attacked, forming, as Sir Wyville Thomson tells us, if the depth is not sufficient to give time for complete decomposition, a calcareous ooze; at greater depths the deep sea muds.* Thus a very great amount of the carbonate of lime in the ocean owes its existence entirely to atmospheric carbonic acid, either from the direct action on calcareous rocks, whether old limestones or silicates,—or indirectly through a series of changes whereby carbonate of soda would be produced, and this being brought into contact with the chloride of lime so abundant in the ocean, carbonate of lime would result. There can be no question but that such effects are going on extensively day by day.

Influence of Vegetation.

If we follow the series of rock-metamorphisms due to the simple absorption of carbonic acid by a plant the result will be seen to be more than interesting. The carbon is assimilated by the plant, an equivalent of oxygen being exhaled. The plant dies, and may become either a part of a coal-bed or may be separately imbedded amongst layers of sediment of some kind. Slow decomposition will now set in, sooner or later, and, if there be a reducible compound near it, chemical changes result. Say the strata contains sulphate of iron: this is reduced to sulphide, commonly known as iron pyrites, a very common mineral in coal-seams—as colliery owners know too well—or in other strata where plants abound. The reduction is effected by the carbon of the plant abstracting the oxygen from the sulphate, and the resulting carbonic acid either is taken up by percolating water, and penetrates farther into the heart of the rock, effecting new changes, and producing carbonates, or it finds its way to the surface through some crevice or by the aid of a mineral spring, and once more mingles with the atmosphere, to be perhaps again absorbed by vegetation, and pass through a round of similar changes afresh. Carbonic

* It now appears, however, that a considerable portion of these muds is derived from the gradual disintegration of pumice and other volcanic *débris* very widely spread over the sea-bottom. See Mr. John Murray's paper on the "Distribution of Volcanic Débris" (Proc. Roy. Soc. Edinb.). The result still due, however, to the action of carbonic acid dissolved in the ocean.

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acid exhalations are very abundant at the surface of the earth, and are in great part ascribable to the oxidation or decay of organic matter which in the first instance derived its carbon from the atmosphere.

The above case shows the result of slow decomposition at great depths; but similar effects are induced by the decay of organic matter near or at the surface. In swampy grounds, lagoons, and deltas, such as those of the Mississippi and the Sunderbunds, the decay of organic matter must exercise a very powerful influence on the chemistry of the soils, rocks, and sediments with which the water charged with the compounds formed during the process of rotting comes in contact. Peroxides, such as those of iron and manganese, will be reduced to the proto state, and will be rendered soluble and carried away in solution, to be after a while re-oxidised and deposited in such masses as to be worth working as ores. Silicates of soda, lime, and magnesia will be decomposed, and removed as carbonates; and sulphates, which are usually present in most waters, will be reduced first to sulphides, and eventually decomposed with evolution of sulphuretted hydrogen. Such a process as this may be observed every autumn in the North of Ireland during the maceration of the flax plant, which is placed in pits filled with water, and, being allowed to remain for some weeks, the softer tissues are rotted away, leaving the fibres fit for manufacture. The stench of sulphuretted hydrogen from the decomposing flax is almost unbearable. Having analysed the mud which subsides to the bottom of the flax-pits, I find that the reducing power of the rotting tissues are as described above. The clay in which the pits are sunk contains nearly all the iron present in the ferric condition when not subject to the action of the plants, but in the mud from the bottom there are only proto-compounds, the iron mostly as carbonate. Nor is there a trace of peroxide of iron in the flax-water, but, on the contrary, plenty of ferrous iron.

Clay-Ironstone.—After this fashion must have been formed the clay-ironstones of the coal-measures. The great swampy estuaries of that period may be regarded as gigantic flax-pits; and the rotting vegetation not only altered other salts and compounds of iron to carbonates, but prevented the oxidation of such carbonate of iron as might have been carried down in solution, until in course of time it also was precipitated along with the clayey sediments.

During such changes near the surface a very large

proportion of carbonic acid is returned to the atmosphere. And that there must be, and always has been, this constant circulation of carbon between the earth and the atmosphere is self-evident. What time it originated must be beyond our ken, but so far back as we have any knowledge of there are evidences in the rocks of vegetable or animal life. And the decomposition of such carbonaceous matters, whether at the surface, immediately after death, or whilst buried under a depth of strata,—as in the case of coal-seams,—has always yielded carbonic acid to the atmosphere. At the same time the carbon returned in this way falls far short of what has been abstracted. But, as Bischof points out, the carbon acts as a carrier of oxygen between the mineral kingdom and the air.

Formerly Greater Abundance of Atmospheric Carbonic Acid.

It has long been considered probable that in remote ages the proportion of carbonic acid was greater than it now is, more especially during the Carboniferous period. The remarkable luxuriance of vegetation of a tropical *facies* during that era, in every part of the globe,—even the polar regions,—indicates a very warm climate universally, and it is also thought to imply a much larger supply of carbonic acid than is now noticeable in the atmosphere. The rarity of warm-blooded animals has been pointed to as corroboration of this view; but strictly this is only negative evidence, the absence of fossil forms affording no proof as to the non-existence in bygone time of animals of any particular type. However, a very curious fact bearing on the question has resulted from Prof. Tyndall's researches on radiant heat. It appears that a very small addition of carbonic acid to air renders it absorptive and retentive of radiant heat, and a slight increase in the percentage of carbonic acid in the atmosphere would have a very distinct result. The visible rays of the sun could pass through the atmosphere to the earth; but the radiant heat from the earth, instead of being dissipated into space, would be imprisoned by the atmosphere, which would thus form a warm envelope around the earth, converting it in fact into an immense greenhouse. The glass roof of a conservatory acts in precisely the same way: it permits the solar rays to penetrate freely, but absorbs and cuts off the escape of the radiant heat, and the interior temperature is thereby rendered tropical. Granting, then, the former abundance of carbonic acid, the extreme richness of the carboniferous vegetation, its tropical character and wide

distribution are very fairly accounted for. I shall show presently that there are other grounds for the supposition that the carbonic acid is now much less than it has been in these far back periods; nor is it to be considered that it reached its maximum even in the Carboniferous age. It is true that the earlier formations afford nothing like such a superabundance of fossil plants; but this has been well accounted for by Dr. Sterry Hunt. He has shown that the vast amount of chemical action that has taken place in the reduction and accumulation of the metalliferous deposits of the older Palæozoic rocks will readily account for the scarcity of fossil vegetation in those rocks. To the decay of plants and the reducing action of the resulting carbonic acid those deposits must be in great measure attributed; and their existence proves that an abundant flora flourished. The manner in which this chemical action takes place will be explained further on. I shall just quote Dr. Hunt's words on this point:—"Where are the evidences of the organic material which was required to produce the vast beds of iron-ore found in the ancient crystalline rocks. I answer that the organic matter was, in most cases, entirely consumed in producing these great results, and that it was the large proportion of iron diffused in the soils and waters of these early times which not only rendered possible the accumulation of such great beds of ore, but oxidised and destroyed the organic matters which in later ages appear in coals, lignites, pyroschists, and bitumens. Some of the carbon of these early times is, however, still preserved as graphite, and it would be possible to calculate how much carbonaceous material was consumed in the formation of the great iron-ore beds of the older rocks, and to determine of how much coal or lignite they are the equivalents."*

If we also reflect that the enormous quantities of limestones which are found in the older formations have been largely dependent on the carbonic acid of the atmosphere—in effect, the further we retrograde towards a primitive condition of things the more directly such carbonic acid must have come into requisition for such purposes, as there would be the less of it stored up in rocks, to be re-utilised as at the present day, when much of the carbonate of lime in waters is obtained by the disintegration of pre-existing limestones—and remember also the carbon that was required for the teeming animal life of ancient times, we shall see that there

* "On the Origin of Metalliferous Deposits."—Chem. and Geological Essays, p. 229.

could have been no lack of carbonic acid; and it becomes a matter of small difficulty to accept the theory that a retrogressively greater proportion of carbonic acid gradually leads back to a primitive atmosphere in which that gas—as well as perhaps other gaseous acids, such as hydrochloric acid—was very abundant.

In regard to this question as to the increase or decrease of carbonic acid, a variety of very interesting points suggest themselves, and the facts almost altogether to range themselves on the side of a progressive decrease of carbonic acid. It seems certain that the amount of carbon stored up in the recesses of the earth very far exceeds that of the entire quantity combined as carbonic acid in the air. It is true that Liebig supposed the carbon so combined, which he calculated to reach 2800 billions of pounds, equal to about 1,250,000,000,000 tons,—figures and tons will probably aid in a better conception of this enormous weight,—to be far in excess of all the carbon stored up in coal-beds, and in plants on and in the globe. But this will hardly be subscribed to when we remember that the coal of the British Isles alone, as estimated by the late Coal Commission, is about 195,000,000,000 tons (I have added about a third for waste, &c., deducted in the original estimate). The carbon in this will weigh about 146,000,000,000 tons, taking an average of 80 per cent. But this was only calculated for coals fit for use, of not less than 1 foot thickness, lying at no greater depth than 4000 feet. Now if we include all the coal of inferior quality, of less than 1 foot thick, and at greater depths than 4000 feet, and then throw into the balance the enormous supplies of coal of the rest of the world and of the older and newer formations, not to speak of the highly carbonaceous shales, slates, schists, and clay ironstones, I think—even taking only this branch of the subject—we should rather be led to agree with Bischof, who, on the other hand, calculates that there is at least 6620 times as much carbon in the earth as Liebig has estimated for the atmosphere;* and Bischof's calculation is based on the very moderate assumption that the average proportion of carbon in all rocks is at least 0·1 per cent, which he considers—and no doubt justly—must fall far short of the real amount. This being so, it would certainly appear that there has been more carbon accumulated in the

* Bischof, Chem. Geology, vol. i., p. 204. Dr. Sterry Hunt has also estimated the amount of carbon secreted in the earth as far beyond that contained in the present atmosphere.

earth than has been restored to the atmosphere by decomposition, and that therefore the quantity of carbonic acid in the air has been gradually lessening from remote periods up to the present time. This appears anything but improbable, remembering the arguments already noticed in favour of the supposed highly-carbonated atmosphere of the carboniferous period; and although the calculations leading to such a conclusion are necessarily based on very imperfect data, it may be safely affirmed, at least, that such a state of affairs is not only possible, but probable.

In these calculations we are not only to consider the carbon of the vegetable kingdom, for it will be obvious that any *animal* carbon which may remain in rocks is also more or less directly derived from the carbonic acid of the atmosphere. Taking the extreme case of the *Carnivora*, it is clear that they must ultimately depend on the air for their supplies of flesh-forming material. Say a tiger dines off a cow; the carbon and nitrogen of her flesh have been obtained from vegetation, which in turn extracted them from the air; so that we have a kind of physiological "House that Jack built." "This is the Tiger that ate the Cow that devoured the Grass that absorbed the Carbon," &c. Viewed in this way it seems that "living on air" is a more substantial kind of existence than has usually been supposed.

Now this which is true of the higher animals applies equally with regard to lower forms. There will be a vegetarian somewhere to fall a prey to a carnivorous marauder, who in his turn may be the victim of a stronger individual; and the successive appropriations may go through any number of steps. Thus the carbon and nitrogen of forms of animal life now fossil have been also derived from the atmosphere. We do not find much, if indeed any, of this carbon in its original form now, or directly traceable to animal agency, because highly nitrogenous organic substances decay very rapidly, but it is not unlikely that their results are to be seen in carbonaceous and bituminous shales, and oleiferous rocks such as those in the neighbourhood of petroleum springs; for, as Dr. Sterry Hunt remarks, since animal tissues contain the elements of cellulose, plus water and ammonia, they may give rise to similar hydrocarbonaceous bodies to those derived from vegetable substances.*

In many cases, also, the decomposition of these animal tissues would result in the formation of carbonates, so that on the whole there must be through this source a vast

* Chem. and Geol. Essays. "On Bitumens and Pyroschists," p. 179.

quantity of carbon—originally drawn from the air—locked up in the crust of the earth. And to all must be added the immense amount of carbon combined as carbonate of lime due to the direct solvent action of atmospheric water on calcareous rocks and minerals. If we add all this to the vegetable carbon already considered, there can hardly be a question but that the amount of carbon abstracted from the atmosphere and hidden away in our globe very, very far, exceeds the proportion present in the air of this age. If this be granted—and I cannot see any possible evasion of it—we must admit that the more ancient atmospheres contained far more carbonic acid than that which now envelopes us, and must renounce the doctrine of Uniformity in this connection at any rate.

Origin of Carbonic Acid.

Having got so far, we are naturally led to inquire as to the origin of the carbonic acid in the first instance. Carbon is so thoroughly associated in our minds with organic matter, or in fact with *life*, that it is difficult to conceive the possibility of its existence in an azoic world, and the difficulty is aggravated by the recollection that the earth must have been at the beginning in a state of incandescence, not to go further and say a gaseous condition. However, under the influence of extreme heat, many elements are isolated which at lower degrees of temperature—but still very great—combine and form chemical compounds. For example, hydrogen and oxygen at a high temperature unite to form water, but at a still higher are again dissociated; and we know that hydrogen exists in a state of incandescence, not combustion, in the sun's photosphere.* Similarly free carbon might have been one of the gaseous constituents of the earth in its nebulous phase,† and as the temperature lowered might have been consumed, or united with oxygen, and gone to form part of the primeval atmosphere. In this way all the carbon now in the crust of the earth would necessarily have been at first confined to the atmosphere. Then when rains began to fall, the carbonic acid, being carried down upon the earth, would soon decompose the silicates which must have resulted from the cooling down of the original heated mass; carbonates would be formed and carried down into the primitive oceans, and clayey residues would be left behind.

* Prof. Henry Draper has just announced the discovery of oxygen in the sun. Nature, August 30, 1877.

† According to Mr. J. Lawrence Smith, carbon in the gaseous form is spectroscopically manifest in the attenuated matter of comets. Am. Journ. Sci., June, 1876.

In course of time, when vegetable and animal life had made their *début*, the withdrawal of the carbonic acid from the air must have proceeded much more rapidly, and the atmosphere gradually cleared to such a condition as to permit of the existence of air-breathing animals. It may be here remarked that the very gradual introduction, in more recent periods, of warm-blooded beings, would also coincide with the hypothesis of the originally highly mephitic state of the atmosphere.

Carbonic Acid now Increasing or Decreasing?

An important question now arises—Is the amount of carbonic acid increasing or decreasing, and what may the result be in either case? To begin with the last part of the question:—Any considerable difference one way or the other must result in a diminution of animal life; in its higher forms in the former event, in all divisions in the latter. Beyond a certain proportion very little above the ordinary standard—at most 10 times, equal to about 5 vols. in 1000,* or 0·5 per cent!—carbonic acid in air becomes a deadly poison to all warm-blooded animals. On the other hand, a diminution in the percentage of carbonic acid would tell even more severely. Vegetable life would languish, graminivorous animals would eventually have nothing to eat, and, finally, the Carnivora, being obliged to prey upon each other, would of course become extinct. And this would be applicable to all divisions of the animal kingdom. The result would be a completely barren and desolate planet, perhaps in some degree resembling the moon. Doubtless that planet has passed through phases of existence alike to those which have obtained upon the earth; and Mr. Proctor† is of opinion that the moon certainly had originally an atmosphere, which is now either altogether absent or is attenuated to an extreme degree. It can well be imagined that this result, and its consequent azoic addition, has been brought about by some such absorption of the constituents of the moon's atmosphere as that which I have endeavoured to sketch out above as regards the earth.

Probable Withdrawal of Oxygen.—It may seem a little paradoxical that such dire effects would more immediately follow the withdrawal of a poisonous gas, and that the latter is on the whole more important to the continuance of life than oxygen gas, which is almost inseparable from our ideas

* WATTS, Chem. Dict., 1862, vol. i., p. 438.
† Quart. Journ. Science, July, 1874. "On the Past History of our Moon."

of existence; but it is undeniable that such would be the case. The blood requires to be oxygenated, but in the absence of carbon there would be no blood at all. And this leads us to another point. The disappearance of carbonic acid must be followed after a period by the withdrawal of oxygen itself. It would gradually be carried by water into the interior of the earth, from which it could make no return, for it would be seized upon by compounds capable of oxidation, and its retreat in the form of carbonic acid would have been cut off.

As to the first part of the question, however, we have as yet no data for its solution. There are several means by which carbonic acid is supplied to the air, and many by which it is removed; but we are not in a position to determine on which side is the predominance, or whether there is at present a balance of power. The principal sources of increase are—

1. Volcanic and other subterranean exhalations.
2. Respiration of animals.
3. Combustion of fuel, &c.

Respecting this last it should be pointed out that we are now restoring to the atmosphere some of the vast quantities of carbonic acid abstracted from it during the Carboniferous period, and imprisoned for ages in the interior of the earth in the forms of coal and clay-ironstone. Perchance by the time we have made an end of our supplies of coal a very sensible difference will have been effected in our atmosphere.

The absorption of the carbonic acid is brought about thus:—

1. By vegetation, as already explained.
2. By the agency of marine organisms which secrete carbonate of lime.
3. By the direct action of atmospheric carbonic acid upon rocks, resulting in the formation of carbonates.

How far these antagonistic processes check each other cannot be conjectured. In order to arrive at any conclusion on the matter we should require to compare trustworthy analyses of air taken at frequent intervals during some thousands of years at least. We have yet no recorded analyses of it older than forty or fifty years. Probably in the remote future information will have been accumulated sufficiently to allow of the solution of the problem; and perhaps in those far distant times a Royal Commission, or some such form of Public Inquiry, will be solemnly convened

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to deliberate as to the possible duration of "Our Carbonic Acid Supplies." But should a necessity ever arise, it is comforting to reflect that it is not likely to occur until some ages after the travelled New Zealander has been gathered to his fathers, and even the very sites of Auckland and Otago perhaps long a subject of curious speculation amongst Central African *savants*. I say it is comforting to take this to heart in these days of sensational cosmogony, when one day we are threatened with destruction from the sweep of a comet's tail, and the next an unfavourable eruption of sun-spots may entail unheard-of miseries upon us. All the information we are in possession of goes to show that the trifling changes that are now observed in the condition of the atmosphere would perhaps require a continuance throughout many millions of years before making themselves disagreeably apparent.

Geological Influence of Oxygen.

This comes next in importance as a geological agent.* I have dwelt first upon the results wrought by the carbonic acid, because the work done by it is immensely greater in proportion to its amount. But oxygen also has its mission. Percolating the rocks, dissolved in rain-water, which is able to absorb a very large quantity of it, it quickly reacts on all oxidisable substances. Carbonates and proto-salts are converted to peroxides; sulphides are changed into sulphates, and sometimes this is accompanied by the production of double salts, such as alums. A familiar instance may be referred to as occurring in the spoil banks of coal-pits, where quantities of aluminous shales, with refuse coal containing iron pyrites, are heaped up together and exposed to the influence of the weather. The oxidation of the iron pyrites results in sulphate of iron, and the sulphuric acid so formed —reacting on the alumina, potash, &c., of the shales—forms a more or less complex alum, which may be observed in small stellate crystals between the laminæ of the shales. Alum slates and earths are very common, and all owe their origin to the oxidation of iron pyrites, or some other sulphide, under circumstances akin to the above.

Ores and Metalliferous Deposits.

The peroxides of iron and manganese are of considerable importance, both commercially and from a scientific point of view. In many cases their formation may be traced

* The amount of oxygen in the atmosphere is about two trillions of pounds (Bischof, *op. cit.*, i., 204), equal to about 892,857,000,000,000 tons.

directly to the action of atmospheric oxygen. In other instances this action is but veiled by a series of complications. Many valuable deposits of iron and manganese are formed in cavities of rocks through the means of water containing carbonic acid and oxygen. The first dissolves the minerals as bicarbonates; then, the excess of carbonic acid escaping as opportunity permits in open fissures, they are oxidised, and deposited at once in an insoluble form, while such other carbonates as happen to be in solution, and which—like lime, magnesia, and the alkalies—have a stronger affinity for carbonic acid than for oxygen, are carried away.

By such a process as this immense beds of limonite have been deposited, and the liberated carbonic acid restored to the atmosphere. Bog iron-ores and the well-known lake iron-ore deposits of Sweden are cases in point. Some of these deposits are assisted by organic agency, some of the Diatomaceæ—*Gallionella* in particular—being very active in this way; but they are only accessory aids, the real work being due to chemical reactions between carbonic acid, oxygen, and soils or rocks. The extensive beds of hematite associated with the Antrim basalts are unquestionably lake-deposits, as Prof. Hull has suggested,* and must be due also to the reciprocal chemical action of the carbonic acid and oxygen from the atmosphere. These beds are now intercalated between the sheets of basalt, and sometimes reach a considerable thickness, consisting of beds of rich ore, poorer ore, and "lithomarge," which is a highly ferruginous clay. Prof. Hull considers that all these were deposited in a large lake or series of lakes. Assuming this, the *modus operandi* was probably this:—The highly ferruginous basalt forming the shores of these lakes being subject to the action of atmospheric water, the iron existing as proto-silicate in the augitic rock, was dissolved out as carbonate and carried into the lake. The excess of carbonic acid then escaping, oxidation ensued, as in the case already referred to, and the iron was precipitated as a hydrated peroxide. At the same time fine sedimentary aluminous matter was also carried down and deposited, and, according as the amount of this was greater or less, a bed of lithomarge or workable ore was laid down. A fresh volcanic outburst eventually taking place, the lakes were covered in, and the ore bed preserved from denudation.

The ore must have been precipitated in the hydrated state,

* Brit. Assoc. Report, Belfast, 1874; also Ex. Mem., Sheets 21, 28, 29 Geol. Surv. Ireland.

and the water of combination was doubtless afterwards given off spontaneously, in the same way as by hydrate of alumina and the hydrated forms of silica. There is indeed considerable analogy between the hematites and the colloid forms of quartz. It is only necessary to compare these pisolitic and botryoidal iron-ores with the calcedonys to see this, and the comparison would be in favour of the aqueous origin of such iron-ores were fresh proof needed.

It will be obvious that the reactions sketched out above with regard to iron ores and compounds applies equally to all other minerals capable of being oxidised or reduced. Copper pyrites, for instance, is often oxidised to sulphate, and the carbonate altered to oxide just in the same manner.

Antagonistic Action of Carbonic Acid and Oxygen.

Clearly, then, the carbon and oxygen derived from the atmosphere sustain antagonistic parts in their action on rocks and minerals. They are perpetually warring the one against the other, and thus keeping up a circulation between the earth and the air. The carbon reduces the oxides whenever it encounters them, and the oxygen replaces the carbonic acid of carbonates with the same inveteracy. The combined effects of these elements in geological transformations is extraordinary when we come to reflect on it. Regarded from an utilitarian point of view, to them we owe probably every metalliferous deposit of value in the world. I have shown how a highly ferruginous rock, such as basalt, containing proto-salts of iron, which are soluble in carbonic acid, might be acted on directly by that acid from the atmosphere. But there are cases where insoluble compounds of iron in small quantity, locked up in rocks, are, by the reducing action of the carbon of decaying vegetation, liberated, and finally accumulated in such quantities as to be of commercial value. Soils and clays contain small portions of peroxide of iron, which is insoluble. The decay of vegetation or other organic matter robs this of oxygen, giving rise to carbonic acid. The resulting *protoxide* is soluble in water containing carbonic acid, or other organic acids, and is carried down into lakes or fissures, where, again absorbing oxygen, it forms beds or veins of hematite.

While insoluble oxides are rendered soluble and allowed to accumulate in this way, soluble sulphates are reduced to insoluble sulphides,—iron pyrites, copper pyrites, zinc blende, galena, &c.,—and, as Sterry Hunt puts it, "removed from the terrestrial circulation," for a time at least. Such

are the processes to which many metalliferous deposits are due.

Another result of the opposition of these two atmospheric gases is the defertilising of soils, and consequent failure of vegetation. An ordinarily fertile natural soil contains, amongst other things, silicates of alumina, lime, potash, and soda, with some peroxide of iron. The silicates of lime and soda will be decomposed by carbonic acid, and the bases removed as carbonates. The potash silicate is also decomposed, and a part of the potash removed by aquatic plants under favourable circumstances, in marshy places, &c.,—conditions under which the vegetation of the Coal era flourished,—and the ferric oxide is reduced to the ferrous state by the deoxidising influence of rotting vegetation. This having occurred, the roots of plants are for a time debarred from any access of oxygen, for any that permeates the soil will be immediately seized on by as much of the proto-compound of iron as has not been carried off in its soluble state, and this is again converted to the higher condition; and these changes continue until they result in the total barrenness of the soil and its ultimate conversion into a hydrous silicate of alumina, almost entirely free from iron, such as we are acquainted with in the fire-clays of the coal-measures—those ancient soils on which the vegetation now forming our coal-seams once grew.*

Ammonia and its Compounds.

Ammonia exists in the air chiefly in the form of carbonate of ammonia, but the quantity, whilst always small, appears to vary greatly, and it is not positively ascertained whether the variation is to be ascribed to natural causes, or ought to be referred to the difficulty of accurate analysis when such small quantities have to be dealt with. It is quite possible, however, that the variability is natural. The minimum recorded is 0·1 part of carbonate of ammonium in one million of air; the maximum is 135 parts.† Rain-water, hail, snow, and dew contain appreciable quantities of ammoniacal

* It is obvious that this only applies to natural soils, since the agriculturist by breaking up the ground affords a supply of oxygen much in excess of what is absorbed by the oxidisable matters present.

† WATTS, Chem. Dict., p. 439. P. Truchot finds that the amount of ammonia varies with the altitude. At Clermont-Ferrand, 395 metres above sea-level, the quantities were 0·93 m.grm. to 2·79 m.grms. in a cubic metre of air,—according as the day was clear or dull,—whilst at Pic de Sancy, 1884 metres, it amounted to 5·27 and 5·55 m.grms. under the same conditions. Comptes Rendus, lxxvii., 1159—1161.

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salts, and in rain from thunder-showers the ammonia is combined as nitrate, the effect of the electric discharge being to oxidise a portion of the nitrogen of the air to nitric acid.*

The atmospheric ammonia is not without its effect on vegetation. It is certain that plants grown in air perfectly free from ammonia never flourish to the same extent as those surrounded by an atmosphere containing some of it; and the experiments of Boussingault, Lawes, and Gilbert—borne out as they are by those of Stöckhart, Peters, and Sachs, and lately by the very conclusive researches of Schlœsing† and A. Mayer‡—show that at least a considerable part of, if not all, the nitrogen of plants is derived from this source. Now the geological connection of this is at once plain, for the decomposition of nitrogenous matter such as plants, in rocks, may lead partly to the formation of nitrates, or, by the evolution of nitrogen and ammonia in volcanic regions, give rise to other minerals, as I shall show presently.

Occasionally the ammonia is absorbed directly from the air by surface mineral matter, as in the case of the volcanic earth of the Solfatara of Puzzuoli. S. de Luca‖ tells us that this contains a quantity of sulphur and arsenic which under the influence of air and moisture form acids, and at once, combine with the atmospheric ammonia. But it is to the decay of vegetation that the vast majority of the nitrogen compounds which are met with, either as minerals or as volcanic emanations, are due, and in whatever state the nitrogen was originally absorbed—whether in the free state or as ammonia—it cannot be doubted that all the nitrogen compounds contained in the earth, as it now exists, are traceable entirely to past and present atmospheres.

The nitrogenous compounds so obtained are themselves subject to an endless variety of changes, in which the gases already described bear no unimportant parts—reducing and oxidising; and these changes, or the effect of heat, may

* Liebig found that of seventy-seven specimens of rain-water, seventeen, collected during thunder-storms, contained nitric acid combined with lime and ammonia. Of the remaining sixty but two contained traces of it.—Bischof, *op. cit.*, i., p. 214. According to Böttger, the induction-spark passed through moist air gives nitrogen peroxide and ozone, but in dry air gives nitrous fumes. —*Chem. Centr.* (1873), 497. Doubtless similar results follow discharges of natural electricity.

† Comptes Rendus, lxxviii., 1700.

‡ Deut. Chem. Ges. Ber., vi., 1404—1413, and Landw. Versuchs. Stat., xvii., 329.

‖ Comptes Rendus, lxxx., 674.

result in a renewed evolution of ammonia to the atmosphere.

Under such circumstances occasionally the ammonia, instead of escaping freely into the air, meets with hydrochloric as in the depths of volcanoes, and combining with it is evolved as chloride of ammonium (*sal-ammoniac*), which is condensed on meeting with the cooler external air. This mineral is often met with in large quantity, so much so, indeed, as to be of commercial value. Thus during the eruption of Vesuvius in 1794 great quantities of this salt were evolved, and it was collected by the peasantry; and Hecla in 1845 yielded very profitable supplies of it. In the vapours of the Solfatara, at Puzzuoli, it is also met with, and it is found mixed with sulphur and other matters in the crater of Vulcano, where it is now being largely collected,* and in considerable quantity at Etna. Then the volcanoes of Kutsché and Turfan, in Central Asia, afford such large supplies that it has been a very valuable article of commerce.†

Prof. Judd is at a loss to explain the production of those large quantities of sal-ammoniac, unless on Daubeny's supposition that nitrogen under the influence of heat is unusually active; but the matter is readily accounted for thus:—The decomposition of nitrogenous organic matter at all times produces ammonia, but especially so under the influence of heat (a familiar instance is the manufacture of coal-gas). That a sufficiency of such organic matter exists in the rocks through which these volcanoes have burst is undoubted, and the ammonia evolved combines with avidity with the hydrochloric acid‡ also given out in volcanic emanations.

Quite lately a new mineral has been discovered incrusting the recent lavas both of Etna and Vesuvius. This is a nitride of iron named "*Siderazote*" by its discoverer, Silvestri,‖ who considers it is due to the decomposition of ammonium chloride by heat in the presence of ferruginous lavas; and although we may not quite accept his theory that the ammonium chloride is formed by the absorption of nitrogen direct from the atmosphere by the lava, it is certain

* J. W. Judd, "On Volcanoes," Geol. Mag., Dec. 2, vol. ii., p. 113.

† Bischof, *op. cit.*, i., 212—213.

‡ The formation of white fumes of ammonium chloride when a glass rod dipped in ammonia is brought near hydrochloric acid will occur to chemical readers.

‖ "The Occurrence of Nitride of Iron amongst the Fumarole Products of Etna, and its Artificial Preparation." Orazio Silvestri, Gazetta Chim. Ital., v., 301—307. Pogg. Ann., clvii., 165—172.

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that the nitrogen has been originally drawn from that source. We may fitly conclude this part of the subject with the mention of the native sulphate of ammonium, *Mascagnine*, of which it may be said that every constituent could have been obtained from the atmosphere.

Nitrogen.

It is obvious that much of what has been said regarding ammonia will apply to nitrogen, but on the whole the latter in its free state appears to have but little influence as a geological agent.

Sulphuric and Sulphurous Acids.

The exceedingly minute traces of these acids make but a slight effect on rocks when compared with the gases already touched upon. That they are not altogether inert may be taken for granted, but both their absorption and re-evolution are of a local nature, being chiefly apparent in the neighbourhood of large towns and about volcanic regions. They may be "withdrawn from circulation" as sulphates and sulphides, and be returned in their original shape, or decomposed into sulphur or sulphuretted hydrogen.

Variations of Atmospheric Pressure.

These cannot but have an appreciable effect on certain classes of geological phenomena. The emanations of gases from the interior of the earth are influenced in some degree. It is well known that explosions in coal-mines sometimes follow a sudden fall of the barometer, which can be well understood on comparing the pressure corresponding to different barometric heights.

Barometer at	28	inches.	Atmospheric pressure	13·70 lbs.
,,	29	,,	,,	14·19 ,,
,,	30	,,	,,	14·68 ,,
,,	31	,,	,,	15·17 ,,

It is usual to refer to the atmospheric pressure as about 15 pounds on the square inch, but the above table shows that a considerable variation makes itself felt within the barometrical range. This must not only control evolution of gases from coal-seams, but also exhalations from open grottoes and caves, mineral springs both thermal and otherwise, and probably from intermittent active volcanoes, such as Stromboli, where the periodical explosion of gases is an

important phenomenon. With regard to this Mr. Judd says "that the barometrical condition of the atmosphere must exercise a powerful influence on such a series of operations as are seen to be going on within the crater of Stromboli, few, probably, would be bold enough to deny." It appears "that the more violent states of activity . . . . coincide with the winter seasons and stormy weather, and its periods of comparative repose occur during the calms of summer, is established not only by the universal testimony of the inhabitants, but . . . . by the actual observations of many competent authorities."* It is hardly necessary to point out that during stormy and wintry weather the barometer is mostly low, while the contrary is the case during summer time and calms.

It is not impossible that similar antagonism between outward and inward pressure may affect the working of many other vents, such as the Solfatara of Naples, and mud-volcanoes, such as those of Sicily, Transylvania, &c.; and that such variations may have no inconsiderable results, both as regards the chemical and cosmical effects of volcanic action.

And now, reviewing the preceding notes, it will be seen what an all-powerful geological agent the atmosphere we breathe is. Without its aid we should know never a stratified formation. The earth would simply form a ball of truly primitive rock, resulting from the cooling down of the original nebulous mass set apart for our globe, the only variation in which primeval and perennial crust being that of the different strata of higher specific gravity towards the interior. We should have no coal, no metalliferous deposits, no rivers or seas, and no rain,—consequently no denudation by "Rain and Rivers,"—for the vapour of water could not ascend into empty space. We should have —— but, last and worst of all, there would be no "we." Life would be impossible, and the earth would finally degenerate into a

—— "pale-faced moon."

That this is probably her ultimate mission cannot be denied. The only consolation is that owing to her larger size, and therefore slower rate of cooling than the moon, she will have gone through a somewhat more extended geological course. There is undoubtedly a very intimate connection between secular cooling and withdrawal of atmosphere, for

* J. W. Judd, *op. cit.*, p. 213.

the cooler the interior the smaller will be the return of gaseous elements to the surfaces; and probably before Saturn and Jupiter have cooled down to a habitable temperature, the senescent earth will roll through space—cold, void, and airless. Sooner or later nothing is more certain than that

—— "to this favour she must come."

III. ON SCIENTIFIC METHOD.*

By M. M. Pattison Muir, F.R.S.E.

WHETHER we turn our attention on ourselves, or seek to pursue the study of mankind in general, or, on the other hand, confine our view to the natural world around us, there is in each case one method by pursuing which we arrive at exact knowledge: that method is the Scientific. What, then, is Science, and what the scientific method? The question, What is Science? is synonymous with another, What is Knowledge? Here is a stone: how do I know it to be a stone? Because it is *like* so many other things which I call stones; it is hard, it possesses a certain colour, it is not easily broken, and so on. I know that it is a stone because I recognise in it certain qualities which I have grouped together and regarded as characteristic of those pieces of matter, to all of which I therefore apply one general name, viz., *stone*. In stones, therefore, there is some quality, or qualities, possessed by all in common, such qualities being sufficient to mark off the possessors of them from all other kinds of matter. Yet these stones may differ from one another in many other ways.

Such a classification is a scientific one. I *know* something about these stones. Were there only one piece of stone in the world I could never know anything about it as a stone. To know we must compare; and the scientific method consists in finding unity amid variety, in tracing the inner relationships between seemingly diverse things (or

* In this paper I have freely availed myself of the stores of information and of the wonderfully original suggestions contained in that most remarkable work "The Principles of Science," by Prof. Jevons, F.R.S.

thoughts), in finding the common link which binds together those things at first sight appearing so widely separated.

This, then is the aim of Science—to know. And to attain this aim she must find the agreements and differences between all things; in other words, she must classify. When we have arrived at a complete system of classification of all phenomena we shall have attained the purely scientific aim of our intellectual existence.

To awaken consciousness there must be more than one phenomenon. Object is compared and contrasted with object, and hereby resemblances and differences are discovered: these are retained in the memory, and compared with other resemblances and differences, as these may be discovered, until at last we are able to find identity amid diversity, to group together a number of objects by means of some great common property, and from this identity to draw inferences which rest on some point of resemblance, and which have for their basis the law that "that which is true of one thing is true of its equivalent." We gradually leave behind us the old idea that ceaseless change is the order of all things; we learn to believe that what we to-day know as Iron will yet be Iron a thousand years hence; we get something definite to reason on, and, step by step, the varied and strange phenomena of Nature are found to be lawful phenomena—are found to have a fixed basis underlying them; until at last we arrive at a general expression for so many and so varied phenomena that we give it the name of a *law of Nature*. Thus we rise from the trivial to the abiding, from the changing to the changeless, from the passing to that which endures, and from that which was capricious to that which is governed by law.

But even here, even in these *natural laws*, we have not attained absolute certainty; they are but general expressions, including a vast number of else isolated phenomena. But what is beyond these phenomena? What is the cause of all these causes? Science, strictly so-called, gives no reply.

It may be urged that modern science teaches that all things are in a continual state of change; that there is no such thing as rest in the physical universe; that every form of energy is but the expression of a change of material particles: but Science, we answer, has gained this knowledge only by grasping the changeless facts underlying the changing phenomena. We do not now simply know that the material universe is constantly undergoing change: we are able, to some extent, to follow the steps of this change; we have reduced the very mutability of Nature to law; we have compared change with change, and in some instances

have succeeded in detecting the connecting-link. We have made a beginning in the classification of the changes of Nature.

If the aim of Science be to detect identity amid variety, it is asked—What means does she employ for accomplishing this end? *Observation, Experiment, and Inference.*

From repeated observations we discover an identity; from a number of identities we infer that what is true of one is true of another; from a number of combined inferences we draw a wider inference, which again we generalise into what is called a law. But before we can establish a law we must make use of the process of deduction—if such or such an inference be true, then this or that phenomenon must follow. Observation or experiment tells us whether the phenomenon does occur or not; if it does, another proof of the correctness of the law is gained; if it does not, there is the less probability that the so-called law is a true one. Thus it is from a series of partial hypotheses which generalise a number of facts that we at last ascend to *the* hypothesis which shall include in its expression all the isolated facts—that is, to a general law. And this method of combined observation, experiment, induction, and deduction is the Scientific Method.

There is nothing pecular in this method; it is but common sense reduced to rule. We are continually and unconsciously guided by the scientific method in our every-day conduct. The countryman who, in the morning, assures his neighbour that it will rain before midday, bases his assurance upon a train of scientific reasoning; he has repeatedly observed that certain appearances of sky, a certain direction of wind, and rain, are associated together: from these observations he has, probably unconsciously, framed the hypothesis that the three sets of phenomena are related together in such a manner that, given the two first, the third is sure to follow; he has proved the value of his hypothesis again and again, by acting on it,—the only scientific method of proving an hypothesis,—and he at last has come to regard it as a law of Nature. But after all it is only an hypothesis, probably a very partial one, and Nature will very likely some day teach him that he has been too ready to narrow her working to the sphere of his own capacities.

The scientific method is applicable, more or less, to all branches of phenomena coming within the scope of human understanding. But the domain claimed by Science is so great as to make it impossible for any man, or body of men, to examine it with completeness. Hence arises the necessity for divisions and subdivisions in Science. The first part which

must be examined is clearly, the laws which regulate reason itself, the laws of thought, and their application to the processes of inference. The study of these belongs to the science of Logic. The application of these principles to reasoning about numbers and quantities constitutes the science of Mathematics, and so on.

We have one set of sciences dealing with physical or material objects; another with mental, moral, or immaterial objects; or with these as they are modified by social relationships. Hence arise the two broad classes of natural or material, and mental or moral science. The scientific method is applicable to both alike, only the questions arising under the second division are more complicated and much more difficult of solution than those under the first. Indeed it is only within recent years that the scientific method has been, in any great degree, applied to the questions of mental and social phenomena.

In endeavouring to classify scientifically the phenomena of Nature we make use of the method, first of all, of Observation.

From experience we gather together a number of facts, but in order to classify these facts we need often have recourse to Experiment.

In the first-named process we do not alter the conditions under which phenomena occur in Nature; we *merely observe* these phenomena as they are presented to us, or at most we *vary our point of view*. In carrying out an experiment, on the other hand, we must carefully vary the *conditions of the phenomenon*, and endeavour, as far as possible, to exclude those which have no influence upon the fact we are studying.

Observation and experiment are the first steps in the ladder leading upward to scientific knowledge.

But where are observations to begin? In our world facts are so numerous, phenomena so almost infinite in number, that no man can say which are to be observed and which neglected. Hence we find that many of the greatest scientific discoveries have taken their rise from what we call "chance" observations. But that which is passed by, by one man, as altogether unimportant, in the hands of another leads to the most important results. The twitching of the leg of a dead frog when accidentally touched by the wires coming from a battery caught the eye of Galvani, and he, following up this chance observation, so added new facts and new theories to our scientific stock that one of the greatest branches of Electric Science now derives its name from this man.

It is impossible, however, to carry out the method of

observation beyond certain limits. Our powers of hearing are not delicate enough to perceive vibrations exceeding somewhere about 38,000 per second; hence if there be sound-producing vibrations quicker than these no amount of observation will enable us to detect them. Not only is this method unable to pass beyond somewhat narrow limits,—it is also liable to lead us to untrue conclusions, unless it be very carefully used. Do we not often hear it said—"See how the buildings of our great ancestors have lasted during the centuries,—strong and firm these old temples, or walls, or roads remain to this day, while the structure which we have raised to-day by to-morrow begins to decay"? Observation tells us that the older buildings remain; observation tells us that the newer quickly disappear; but observation does not tell us that it is only the great buildings of antiquity which remain—the buildings which, from their purpose and design, we should expect to be *very* strongly put together; and that the ordinary houses and the common buildings have all *long ago* utterly disappeared. The conclusion drawn from observation alone—viz., that our ancestors built more strongly than we do—is therefore a conclusion which is not proved by the evidence adduced.

Again, the mind of the observer may be so overcast with prejudice or fancy, or may be so dim and dull as either not to receive aright the image of outward things, or to transpose that image so that it becomes a caricature, not a truthful picture. He who when shown, in the old heathen temple, the picture of all those who had been saved from shipwreck after paying their vows, and asked to believe *now* in the power of the gods, replied "But where are they who paid their vows and were not saved from shipwreck?" was a man whom we have often need to copy.

By *observation alone* we cannot tell the *exact* conditions regulating the occurrence of any phenomenon; these conditions can be determined only by *experiment.* But where an observation has been made, and we are wishful to determine the exact conditions which regulate the occurrence of the observed fact, we shall find that it is very difficult to fix on these conditions. Every fact is so closely related to so many others that it becomes very hard to strike out those conditions which are really non-essential. Nay, it often happens that what at first sight appears to be the most important condition of a phenomenon proves, after experiment, to have little or no influence on this phenomenon while other overlooked circumstances are the true governing causes. Thus if we let fall a piece of lead and a sheet of

paper of the same weight, from a height, we find that the lead reaches the ground long before the paper does. We should naturally conclude—as all, or almost all men did before the time of Galileo—that the *nature* of the two bodies influences the velocity of their descent; whereas it is actually found, by carefully conducted experiments, that no property of bodies except their absolute mass has any influence upon their gravitating powers. In experiment we must therefore seek to eliminate, one by one, those circumstances which are really not of importance as influencing the phenomenon in question; we must simplify the experiment as far as possible, taking care, however, that in our attempts at simplification we do not overlook *the* circumstance really governing the phenomenon. After all, however, some overlooked condition may be present, the non-observance of which entirely vitiates our results. Experiments were long ago very carefully carried out with a view to prove that earth can be formed from water: water was again and again distilled from a perfectly clean glass vessel, yet there remained a small quantity of a white earth in the vessel in which the water was distilled. If the water has not been converted into the white earth, where has this substance come from? The conclusion seemed inevitable, and the conclusion was therefore adopted —water may be converted into earth. But the overlooked circumstance was this:—Water acts on glass, especially at high temperatures, so as to dissolve part of it, and the white earth is really a portion of the glass vessel dissolved by the boiling water, and left in the vessel when the water has been entirely boiled away.

An exceedingly instructive example of the process of elimination of non-important conditions of experiment is afforded by Sir Humphrey Davy's researches upon the electrolysis of water. When water was decomposed by the electric current, an acid and an alkali invariably made their appearance at the poles along with the oxygen and hydrogen. Electricity, some people supposed, caused the production of the acid and the alkali; others imagined that water always contains acid and alkali. By using agate or gold vessels in place of glass, to contain the water, Davy showed that less acid and less alkali was produced. Finally, by carrying out the decomposition in gold vessels, in the exhausted bell-jar of an air-pump, he was able to obtain from pure water, oxygen and hydrogen free from both acid and alkali, thus showing that the presence of air was in some way the cause of the production of the acid and of the alkali.

If, therefore, an experiment seems to point irresistibly to

this or that conclusion, we must be very chary in accepting this result until we have again and again varied the conditions of the experiment so as to bring under notice every circumstance which can in any way influence the phenomenon we are investigating.

To discover what condition may or may not influence a given phenomenon becomes therefore one of the most important problems of the scientific investigator. And here the man of a keen insight and quick apprehension has a very great—in fact, an immeasurable—advantage over the ordinary dull and plodding experimenter.

There seems to be a somewhat widespread idea that there is no longer any use for genius; that magnificent laboratories, elaborate organisation, Government endowment, and certificated teachers are to carry all before them, and achieve results such as the world has never seen. To me it seems that now, as ever, genius is necessary for any really great discoveries; that no amount of training, nor of organisation, nor of artificial selection, can make up for the absence of native talent—of that subtle, scarce definable something, which we call genius.

If the genius is there, by all means let us educate it as best we may; let us also do our utmost to train all, whether possessed of genius or not,—for in doing this we shall, at any rate, be giving to all the means of leading a nobler and a more useful life than they otherwise could; but let us beware of thinking that *we* can evoke this rare and wonderful product, genius, by any method of selection, or by any system of competitive examination: plodding, persevering, patient work is of the very utmost importance; but the power of grasping the one true condition of a problem, to the exclusion of the many trivial but *seemingly* important conditions, is of yet more importance; while he who combines both of these qualities,—he who has genius to see and patience to follow,—he it is who stands forth as the great discoverer, as the poet of Science. The successful investigator of Nature must be patient; he must very often reserve his judgment until experiment proves to him that this or that conclusion must be the right one; he must be ready to frame hypotheses, but he must not shrink from submitting these to the most rigorous experimental test, and when he finds experiment and hypothesis opposed he must be ready to doubt the latter, but yet not despair of finding another truer generalisation; he must never disdain help, even from the humblest; he must have no envy; he must neglect no objection; he must not choose and *then* compare, but after

comparing many times he must choose; and while he is thus humble he must not hesitate to frame hypotheses,—he must risk something in his search for truth, knowing that in rigorous experiment he has a means of trying his queries whether they be true or not. "The philosopher," says Faraday, "should be a man willing to listen to every suggestion, but determined to judge for himself. He should not be biassed by appearances; have no favourite hypothesis; be of no school; and in doctrine have no master. He should not be a respecter of persons, but of things. Truth should be his primary object. If to these qualities be added industry, he may indeed hope to walk within the veil of the temple of Nature."

I have said that we still need men of genius who can see through the tangled web of facts and catch a glimpse of the governing power behind. But it may be asked, Is not this a different method from that of strictly inductive reasoning recommended by the great father of true logic, Bacon himself? Yes, it is somewhat different; yet I think that a due attention to historical facts will show us that without deductive reasoning no true generalisations have ever been reached in Science.

By an examination of facts alone we gain empirical knowledge; scientific unity can only be gained by embracing these facts within one general principle. A mere collection of empirical facts does not constitute scientific knowledge; we must explain these facts,—that is, we must take out the folds, "*Ex plicis plana redere*," we must show the resemblances, more or less deep, between the facts; so long as a fact remains alone, unattached and seemingly unattachable to any other, we feel a certain uneasiness, unsatisfiedness, in regarding this fact; and such an uneasiness *may have*—in certain ages *has*—developed into a superstitious dread of the unexplained fact. In other ages than the present the sweep of the comet across the sky was regarded as an omen of evil,—it was an awful unexplained fact,—but now that we know that the laws governing the movements of the comets are the same as those which rule the calm and peaceful stars, we no longer experience any dread at the approach of these, once fearful, visitors. But the man of Science is often taunted with his lack of awe and reverence of the mysteries of Nature: the accusation is, I believe, only made—if made in earnest—by those who cannot take the trouble of investigating Nature for themselves; by those who think it a grander thing to speak of mystery, and greatness, and reverence, than to exhibit those qualities in themselves which

they demand in others. To quote the words of Charles Kingsley:—"There is a scientific reverence, a reverence of courage, which is surely one of the highest forms of reverence, that, namely, which so reveres every fact that it dare not overlook or falsify it, seem it never so minute; which feels that because it is a fact it cannot be minute, cannot be unimportant; . . . and which therefore, just because it stands in solemn awe of such paltry facts as the Scolopax feather in a snipe's pinion, or the jagged leaves which appear capriciously in certain honeysuckles, believes that there is likely to be some deep and wide secret underlying them which is worth years of thought to solve. But as for that other reverence which shuts its eyes and ears in pious awe . . . what is it but cowardice, very pitiable when unmasked; and what is its child but ignorance, as pitiable, which would be ludicrous were it not so injurious?"*

To return to the main subject. We wish for hypotheses which shall explain our observed or experimentally determined facts. If we are determined to do without hypotheses in our scientific method, let us see what is required of us. Given two circumstances, and one hundred other distinct circumstances which may possibly be connected with these, we are required to find, by mere inductive reasoning, the law regulating the coincidences existing between these circumstances. Now there are no less than 4950 pairs of circumstances, under the conditions just named, between which a coincidence may exist. We shall therefore be required to try these 4950 cases, in order to determine which of them represents the true grouping of the connected circumstances. Would it not be easier, after attentively looking at all the circumstances, to say, probably the coincidence lies here, and then try whether it does or not?

As an illustration of the vast number of combinations possible under certain circumstances the following is instructive:—In whist, four hands of thirteen cards each are simultaneously held: "The number of distinct possible deals is so great that twenty-eight figures are required to express them. If the whole population of the world—say a hundred thousand million persons—were to deal cards day and night for one hundred million years, they would not have exhausted in this time one one-hundred-thousandth part of the possible deals."† If we do not know anything of hypotheses, do not hazard at the least a guess, we shall very

* "Science." A Lecture delivered at the Royal Institution.

† Essay on Probability, by LUBBOCK and DRINKWATER, quoted by JEVONS, Principles of Science, vol. i., p. 217.

probably find ourselves—like the alchemists—spending our years in useless labour, searching in the labyrinth of detached facts without any guide, and, like them, we shall arrive at no true results.

The doctrine or theory of combinations enables us to determine the possible number of ways in which a given set of facts or circumstances may be grouped together. This theory is of the utmost importance in enabling us to form just conceptions of the nature of the task set before him who would investigate Nature. The importance of the doctrine of combinations is thus insisted on by James Bernouilli:*—"It is easy to perceive that the prodigious variety which appears both in the works of Nature and in the actions of man, and which constitutes the greatest part of the beauty of the Universe, is owing to the multitude of different ways in which its several parts are mixed with, or placed near, each other. But because the number of causes that concur in producing a given event, or effect, is oftentimes so immensely great, and the causes themselves are so different one from another, that it is extremely difficult to reckon up all the different ways in which they may be arranged or combined together, it often happens that men, even of the best understandings and greatest circumspection, are guilty of that fault in reasoning which the writers on logic call *the insufficient or imperfect enumeration of parts or cases;* insomuch that I will venture to assert that this is the chief and almost the only source of the vast number of erroneous opinions—and these, too, very often in matters of great importance—which we are apt to form on all the subjects we reflect upon, whether they relate to the knowledge of Nature, or the merits and motives of human actions. It must therefore be acknowledged that that art which affords a cure to this weakness or defect of our understandings, and teaches us to enumerate all the possible ways in which a given number of things may be mixed and combined together, and that we may be certain that we have not omitted any one arrangement of them that can lead to the object of our enquiry, deserves to be considered as most eminently useful and worthy of our highest esteem and attention, And this is the business of the *art or doctrine of combinations*. Nor is this art or doctrine to be considered merely a branch of the mathematical sciences, for it has a relation to almost every species of useful knowledge that the mind of man can be employed upon. It

* De Arte Conjectandi, translated by Baron MASERES (London, 1795, p. 353), quoted by JEVONS in Principles of Science, vol. i., pp. 198—200.

proceeds, indeed, upon mathematical principles in calculating the number of the combinations of the things proposed; but by the conclusions that are obtained by it the sagacity of the natural philosopher, the exactness of the historian, the skill and the judgment of the physician, and the prudence and foresight of the politician may be assisted, because the business of all these important professions is but *to form reasonable conjectures* concerning the several objects which engage their attention, and all wise conjectures are the results of a just and careful examination of the several different effects that may possibly arise from the causes that are capable of producing them."

When we apply this theory to facts we are astonished at the results. Speaking of "the variety of logical relations which may exist between a certain number of terms," Prof. W. Stanley Jevons* says:—"Four terms give 16 combinations, and no less than 65,536 possible selections from these combinations; . . . for six terms the corresponding numbers are 64 and 18,446,744,073,709,551,616. Considering that it is the most common thing in the world to use an argument involving six objects or terms, it may excite some surprise that the complete investigation of the relations in which six such terms may stand to each other should involve an almost inconceivable number of cases. Yet those numbers of possible logical relations belong only to the second order of combinations."

If the facts of Nature be so numerous we can never hope to know them all. Perfect knowledge is for us impossible: how, then, are we to make the most of that partial knowledge which we can alone attain to? How can we measure the extent of our knowledge of any subject? By means of the theory of probability.

Although perfect knowledge is impossible, yet we cannot be content with the accumulation of mere isolated facts. We attempt to group facts together, to form theories, and to apply these to the explanation of newly discovered facts. The theory of probability must guide the mind in guaging its knowledge of any group of facts. And the theory of probability is, as Laplace has said, "good sense reduced to calculation."

Suppose it be required to determine the atomic weight of an element, we devise various methods of measurement, we repeat the measurements again and again, but there are nevertheless errors inherent in each method, errors in the

* Principles of Science, vol. i., p. 223.

instruments employed, and errors in our readings of these instruments, &c. The result can never be more than approximately correct, and the results obtained by the different methods will not be exactly the same. We do not therefore *know* the true atomic weight of the element; but the theory of probability enables us to assign to each result its comparative trustworthiness, and so to deduce the numerical probability of the average result being absolutely correct.

The application of the theory becomes often very difficult. Our knowledge is at the best so limited that it is difficult to assign to two propositions their relative probabilities. When we deal with simple numbers, as those obtained in the illustration given, we can apply the theory with comparative ease; but when we come to more complicated questions in physical science we find it almost impossible to obtain sufficient, and sufficiently reliable, data to enable us to estimate probabilities. But, as Prof. Jevons has pointed out, "Nothing is more requisite than to distinguish carefully between the truth of a theory and the truthful application of the theory to actual circumstances. As a general rule, events in Nature or Art will present a complexity of relations exceeding our powers of treatment. The infinitely intricate action of the mind often intervenes, and renders complete analysis hopeless. If, for instance, the probability that a marksman shall hit the target in a single shot be 1 in 10, we might seem to have no difficulty in calculating the probability of any succession of hits: thus the probability of three successive hits would be one in a thousand. But, in reality, the confidence and experience derived from the first successful shot would render a second success more probable. The events are not really independent, and there would generally be a far greater preponderance of runs of apparent luck than a simple calculation of probabilities could account for. In many persons, however, a remarkable series of successes will produce a degree of excitement rendering continued success almost impossible."

We must be content with partial knowledge.

In ascending from facts to generalisations, which generalisations are more or less probably true, we must make use of hypotheses; we must accumulate facts, make an hypothesis to explain them, and test the hypothesis by appeal to facts. The investigator of Science must begin with facts; he must end with facts; but between the two he must interpolate hypothesis. He looks at a number of facts; gradually he sees, or thinks he sees, a light dawning on him—a central idea, round which all the facts group themselves in a

luminous whole. But he does not stop here; he again appeals to facts. He says "If my hypothesis be correct, this or that fact must follow." Then he tries experiment. Is the predicted fact really a fact? By the test of experiment he is content to abide; he knows that Nature—however hard sometimes it is to make her answer at all—never answers except truly.

Hypotheses must be used in Science, but hypotheses may be abused. What, then, are the marks of a good hypothesis?

An hypothesis must be workable; it must not go against any well-established scientific generalisation, and it must be ready to submit to have its predictions proved by strictly experimental methods.*

A good scientific hypothesis must be workable; that is, it must allow us to make determinate predictions—predictions which can be proved or disproved by experiment. A vague generalisation, which does not allow of definite deductive reasoning, can have no place as a scientific hypothesis.

A good scientific hypothesis must not be opposed to any well-established generalisation of Science. This statement may probably be called in question by many. It is no uncommon thing to find people talking of the way in which Science sweeps aside all preconceived ideas, all Old World notions, all long-cherished delusions. And this is very true; only these people, I am afraid, forget the other side of the case; they forget that did Science present us with nothing but change succeeding change, doctrines swept away and replaced by others to be themselves removed as Science advances, Science would have no claim on our acceptance. It is because Science is at once prolific of changes, and conservative in the extreme, that she has accomplished her work in the world.

We know so little of Nature that we must be ready at any moment to give up that which we had supposed we did know; and yet we have such trust in the stability of Nature that we must cling to those theories which have been gained by slow accumulation of facts, until there is absolute experimental proof of their falsity.

Such a theory as that of the Conservation of Energy is the general expression of a vast number of facts: it explains these facts; it is a well-established generalisation of Science. If we are seeking to explain a number of newly-discovered facts, it is evidently our duty to frame an hypothesis which shall not be itself out of keeping with this theory of the

* Principles of Science, vol. ii., p. 139.

conservation of energy. For if we do not do so we are very probably assuming the incorrectness of that great body of facts upon which the theory rests. It would in most cases be almost better to distrust our personally-observed facts than to distrust so well-founded a generalisation. This is one view to take of the question. But, on the other hand, the theory of the conservation of energy is a theory only: it is probably true; we do not, and cannot, know whether it is or is not certainly true. If the observed facts, after the most careful observations, still remain unmoved, and if they are apparently opposed to the generally-accepted theory, the better method will doubtless be complete reservation of judgment until further experimental data is forthcoming. If the observed facts are, however, absolutely opposed to the theory, and if these facts cannot be gainsaid, then the theory must go; it has done its work, and must be supplanted by a wider generalisation. The scientific investigator must therefore cling to theory, and yet be ready to abandon theory at the call of fact.

It is, it seems to me, of the utmost importance to insist on this view of the work of the student of Nature; to declare that he trusts Nature altogether, but he distrusts his own powers of comprehending the workings of Nature; that he feels that all things are changing, but he nevertheless clings to what he can grasp of the changeless. The frame of mind of the man of Science is, then, at once opposed to those who would have us believe that "victorious analysis" has now at last reduced all things under her feet, and to those who would have us accept the teaching of authority in place of the teaching of facts. Both alike assume a vast amount of knowledge which neither is possessed of. But the last characteristic of a good scientific hypothesis is its readiness to submit to have its predictions proved by strictly experimental methods. Every newly-discovered fact which is capable of explanation in terms of an accepted hypothesis adds something to the *probable* truth of that hypothesis. Every newly-discovered fact which cannot be explained in terms of the hypothesis takes away something from the probable truth of the hypothesis. We may observe facts which are apparently opposed to the hypothesis which we have provisionally accepted, and yet we may not be justified in condemning the hypothesis, because these facts may either be but partially examined by us, or the hypothesis may not have been fully grasped in all its bearings. But if the hypothesis is to hold its ground there must be no experimentally demonstrated fact, the existence

of which would be impossible were the hypothesis correct. To take an instance:—The upholders of the Phlogistic theory affirmed that when a metal is burned it parts with phlogiston; that the product of combustion is metal *minus* phlogiston; and that the re-transformation of the product of combustion into metal is brought about by the absorption of phlogiston. The upholders of what might be called the Oxygen theory affirmed that when a metal is burned it combines with oxygen: that the product of combustion is metal *plus* oxygen; and that the re-transformation of the product of combustion into metal is brought about by the removal of oxygen. Each hypothesis had facts in its favour; each explained many facts. But the fact discovered by Davy, in 1807, that the metals potassium and sodium are actually produced by the removal of oxygen from those substances which are themselves formed when these metals are burned, could *not* be explained in terms of the phlogistic theory. Either the fact or the theory must give way. The fact was established beyond a doubt; therefore the theory—in its then accepted form at any rate—had to succumb.

A good scientific hypothesis must, then, be in keeping with facts; but it does not follow that it must be simple, or that it must make no claims upon our belief. The hypothesis which well explains the facts concerning light is, we might almost say, absurd in the demands which it makes upon our credulity. "We are asked by physical philosophers to give up all our ordinary prepossessions, and believe that the interstellar space which seemed so empty is not empty at all, but filled with *something* more solid and elastic than steel. As Dr. Young remarked, 'the luminiferous ether pervading all space, and penetrating almost all substances, is not only highly elastic, but absolutely solid.' Sir John Herschel has calculated the amount of force which may be supposed, according to the undulatory theory of light, to be exerted at each point in space, and finds it to be 1,148,000,000,000 times the elastic force of ordinary air at the earth's surface, so that the pressure of the ether upon a square inch of surface must be about 17 billions of pounds. Yet we live, and move without appreciable resistance through this medium, indefinitely harder and more elastic than adamant. All our ordinary notions must be laid aside in contemplating such an hypothesis; yet it is no more than the observed phenomena of light and heat force us to accept."*

* Principles of Science, vol. ii., p. 145.

Again, the hypothesis of Gravitation forces us to believe that a particle of matter here, on this earth, is at this moment acting upon each other particle of matter in the universe, and that apparently with an action to which time counts as nothing, and the mass of all the planets as a thin screen offering really *no* opposition.

When we come to examine the hypotheses of Science, we find that they have been developed to very varying degrees of perfectness. "Where, as in the case of the planetary motions and disturbances, the forces concerned are thoroughly known, the mathematical theory is absolutely true, and requires only analysis to work out its remotest details. It is thus in general far ahead of observation, and is competent to predict effects not yet even observed, as, for instance, lunar inequalities due to the action of Venus upon the Earth, &c., to which no amount of observation, unaided by theory, would ever have enabled us to assign the true cause. . . . Another class of mathematical theories, based to a certain extent upon experiment, is at present useful, and has even in certain cases pointed to new and important results which experiment has subsequently verified. Such are the dynamical theory of heat, the undulatory theory of light, &c. . . . . A third class is well represented by the mathematical theories of Heat (conduction), Electricity (statical), and Magnetism (permanent). Although we do not know *how* heat is propagated in bodies, nor *what* statical electricity or permanent magnetism are, the laws of their forces are as certainly known as that of gravitation, and can therefore, like it, be developed to their consequences, by the application of mathematical analysis."*

If it be impossible to group together the facts of Nature in every possible combination, and then to infer general laws; if it be necessary to make use of hypotheses, it may be asked—Is there no method applicable for forming these hypotheses? nothing to guide us in our guesses at Nature's laws? Of course it would be impossible to lay down *rules* for making hypotheses, just as it would be absurd to *teach* a man to be a genius; nevertheless, if we study the trains of thought by which the most eminent naturalists have been led to their great discoveries, we can arrive at some general idea of the methods which they have followed. These discoveries have evidently been guided by analogy. From one similarity, or from a few similarities noticed between different substances or between different sets of facts, they have

* Thomson and Tait, The Oxford Pamphlet, p. 110.

inferred the existence of more points of similarity; they have then framed hypotheses which have guided them in their subsequent experimental investigations. To take an illustration:—When an electric machine was worked a peculiar smell was noticed; when a stick of moist phosphorus was allowed to remain exposed to air, a similar smell was perceived; when a hot glass rod was dipped into a mixture of ether vapour and air, a similar phenomenon was perceptible. From these observed similarities Schönbein inferred that the cause of the peculiar smell was probably the same in each case, and following up this analogy by experimental investigation he discovered ozone—a substance which has played, and is doubtless destined to play, a most important part in general chemical theory.

Many instructive instances of the application of analogy are to be found in the science of chemistry; in fact that science is almost entirely founded on more or less general laws which have been deduced by analogical reasoning. The fact that certain elements form groups having many common properties, and more or less sharply differentiated from other groups, has long been known. The further fact that there is, in many instances, a regular gradation in the atomic weights of the members of such groups, seemed to point to a connection between atomic weight and general chemical behaviour of the elements. Many facts were in keeping with this assumption. The connection between chemical properties and change in atomic weight has of late years been much attended to; and it has been shown by Mendelejeff and others that, if the elements be arranged in order of their atomic weights, beginning with that which has the least atomic weight, the general properties—not only of the elements, but also of their compounds—may be regarded as functions of the atomic weights; that, moreover, these functions are periodic,—that is, that groups of elements may be formed in order of increasing atomic weights, and that the general relations existing between, say, the third member of group two and the other members of the same group correspond with those relations which exist between the third member of group four and the remaining members of this group. Following up the analogy, Mendelejeff has propounded an hypothesis which goes under the somewhat ambitious title of the *periodic law*, and from this hypothesis he has made certain predictions. Among other predictions he has foretold the existence of elementary bodies other than those we are acquainted with: he has even ventured to assign certain properties to some of these

hypothetical elements. Nor have his predictions been altogether unfulfilled. The most recent addition to the chemical elements is the metal gallium: in very many of its properties—in fact in its general chemical behaviour, so far as this has been experimentally examined—gallium corresponds very closely with one of Mendelejeff's hypothetical elements. We have here an example of an hypothesis founded on analogical reasoning.

But analogy may mislead; it has often misled men in framing hypotheses. As telescopes were made of greater and greater power, astronomers found that the nebulæ were resolved into clusters of stars. One by one these apparently gaseous masses were proved to be really aggregations of solid matter. Analogy suggested that all nebulæ would be resolved when sufficiently powerful instruments could be brought to bear upon them. But meanwhile a new method of research was discovered; and by the use of spectrum analysis Huggins has proved that certain nebulæ really consist of gaseous matter, and has therefore shown that the analogy in the structure of these bodies was not so complete as was supposed.

Analogy must evidently be used with caution. And here again we perceive the need of genius in Science. The ordinary man may amass facts, may even trace out a few analogies between groups of facts, but it is only the man of genius who will discover *the* analogy which will guide to great generalisations. Very probably even the genius will follow many false scents; but if he be a true student of Nature, besides being possessed of the divine gift of imagination, he will test his hypotheses framed on analogical reasonings by appeal to facts, and he will discover the true analogy and frame the correct hypothesis at last.

Of the vast masses of facts which are presented to the enquirer in each branch of Science there will be some of more value—considered as guides in deducing general laws—than others. Not unfrequently it happens that it is the fact which somehow refuses to fit in with the generally accepted hypothesis which becomes the means of guiding the investigator to a new and wider hypothesis. "When, in an experiment, all known causes being allowed for, there remain certain unexplained effects (excessively slight it may be), these must be carefully investigated, and every conceivable variation of arrangement of apparatus, &c., tried, until, if possible, we manage so to exaggerate the residual phenomenon as to be able to detect its cause. It is here, perhaps, that in the present state of Science we

may most reasonably look for extensions of our knowledge: at all events we are warranted by the recent history of natural philosophy in so doing."*

As an illustration of the use made by genius of "residual phenomena" I might cite the discovery of the planet Neptune by Adams and Le Verrier. Slight anomalies were observed in the motions of Uranus: these were studied; the hypothesis was framed that the peculiar movements were due to the presence of an unknown body; observations were carried out, and the new planet was discovered.

Almost every science presents us with residual phenomena awaiting explanation. To mention one in chemical science. Why are the densities of the vapours of phosphorus and arsenic twice as great, and the densities of the vapours of mercury and cadmium one-half as great, as all analogical reasoning would lead us to imagine they should be? Here is an unexplained fact which will doubtless one day be prolific of consequences.

I have thus attempted to sketch the main points in that method which has been, and is, pursued by scientific men in their attempts to discover the truths of Nature: in conclusion I must say a few words regarding the limits of scientific method.

In science we start with facts, we then form hypotheses which we test by appeal to facts. But so great is the number of facts presented to us that we cannot observe or experimentally determine more than a small, almost an infinitely small, portion of them. Much less can we hope to form satisfactory hypotheses which shall explain them all. This is true in physical science. The mass of facts gathered together by the naturalist is already extremely large; but there can be no doubt that the number of the unknown vastly exceeds that of the known facts of Nature. And of the known facts how few have as yet been explained. The problem of the "mutual effects of three bodies, each acting on the other under the simple hypothesis of the law of gravity," can scarcely be said to be yet completely solved. And if this comparatively simple case has puzzled the ingenuity of the mathematicians what are we to say to the application of mathematical processes to the explanation of those motions and mutual actions which we have reason to believe are being performed and undergone by the constituent portions of every chemical atom? Each of these particles, Sir J. Herschel has remarked, is continually solving differ-

* Thomson and Tait, The Oxford Pamphlet, p. 108.

ential equations, which, if written out in full, might perhaps belt the earth.

In physical science our ignorance is practically infinite as compared with our knowledge; and when we come to mental and moral phenomena we are almost without any data on which to base strictly scientific reasoning. Each human being presents the phenomenon of a mass of conflicting hopes, fears, desires, passions, and inclinations which science can never hope to classify. How shall we measure these mental phenomena? How shall we weigh accurately the emotions even of the least emotional of human beings? What units shall we employ? How shall we calculate the effects of each human life upon the general life of the community? We cannot hope ever to reduce these things within the grasp of rigid quantitative analysis. As Prof. Jevons has truly remarked:—"As astronomers have not yet fully solved the problem of three gravitating bodies when shall we have a solution of the problem of three moral bodies?"* And shall "victorious analysis" ever dream of attempting to bring under her formulæ the facts concerning man's relation to the physical world around him? If each set of phenomena, physical and mental, considered apart from the other, far surpasses our powers of investigation, how can science ever hope to approach the problem of the mutual relations of the two? "The air itself is one vast library, on whose pages are for ever written all that man has ever said or even whispered. These, in their mutable but unerring characters, mixed with the earliest as well as the latest sighs of mortality, stand for ever recorded—vows unredeemed, promises unfulfilled, perpetuating in the united movements of each particle the testimony of man's changeful will."† We cannot solve the mystery of the physical world, nor the mystery of the mental world, nor the mystery of the connection between the two.

But we do attempt nevertheless to lessen the sphere of our ignorance and to change the unknown into the known. We endeavour to explain facts by grouping them together under a generalisation. The wider generalisations of science are generally called laws. Having made a bold generalisation, having appealed to facts and found that our generalisation stands the test in any instance, we are very liable to conclude that this generalisation *must* hold good in all cases, and to give to the expression a *coercive* value. Indeed, the name *law* almost

* Principles of Science, vol. ii., p. 458.

† CHARLES BABBAGE, Ninth Bridgewater Treatise, p. 113.

implies coercive power. But are we justified in doing this? To say that the law must hold good in all cases implies infinite knowledge: we may have proved the law to apply in every instance which we have examined, but there is the chance that in the next instance it will fail. Prof. Jevons shows that "no finite number of instances can warrant us in expecting with certainty that the next instance will be of like nature." Every fresh instance of like nature to the preceding increases the probability that the law will hold good in all instances, but after all it is only a probability that we have gained. "The laws of Nature, as I venture to regard them, are simply general propositions concerning the correlation of properties, which have been observed to hold true of bodies hitherto observed. On the assumption that our experience is of adequate extent, and that no arbitrary interference takes place, we are then able to assign the probability, always less than certainty, that the next object of the same apparent nature will conform to the same law."*

We speak of matter obeying the law of gravity. In this proposition we imply the existence of two things—matter and force; matter, a *something*, acted on by *another something*, force. Of these two things we cannot give very good definitions. Matter is "that which can be acted upon by, or can exert force:" and force is "any cause which tends to alter a body's natural state of rest, or of uniform motion in a straight line."† But the force of gravity acting on particles of matter does not necessarily cause the actual approach of one body towards another; the action of this force upon a given particle of matter is conditioned by the number, mass, distance, and relative position of all the other particles of matter within the bounds of space at the instant in question. We must not forget that the action of the laws of Nature upon the matter of the universe is dependent upon the *collocations* (as Dr. Chalmers expressed it) of that matter at any moment of time. Given the same laws and the same mass of matter, but let the initial collocations of that matter vary, then the results would be altogether different for each collocation. No single law of Nature can be supposed to act independently of other laws. Every law is conditioned in its action by other laws. Or, perhaps, we should say that, in our ignorance, we are obliged to speak of special laws acting and reacting upon one another, when to infinite knowledge all would appear as under the control of but one

* Principles of Science, vol. ii., p. 431.
† Thomson and Tait, The Oxford Pamphlet, pp. 53, 54.

law. But by us, at any rate, various laws must be recognised; and these are mutually related. Now if we cannot hope to know all the facts of the Universe still less can we hope to comprehend all the laws thereof, and much less can we dream of arriving at a knowledge of the mutual actions of those laws upon one another, and the modifications in the action of one law upon material objects introduced by the interference of another law, or of other laws. And even our knowledge of individual laws is but approximative: the more carefully Nature is examined the more reason have we for disbelieving in the simplicity of her actions. At first everything appears chaotic; then facts group themselves together, generalisations are made, laws are framed. But after a time, as investigation proceeds, and as more exact methods are introduced, the law is found to be not quite in keeping with facts; the formula was only approximately true. There are slight exceptions, so slight that the older and ruder methods of research failed to detect them: the law is not rigorously exact. In the hands of the trained and able naturalist these small exceptions often prove stepping-stones to higher generalisations, which embrace in their enunciation the less widely applicable generalisation. But if every improvement in our methods of research serves to point out exceptions to what were formerly accepted as general laws, are we entitled to assume that we have *now* reached the true generalisation? Would it not be more becoming the spirit of true science to acknowledge our ignorance, to remember that while we have made one step nearer the goal that goal is itself still at an infinite distance from us?

I might illustrate this subject by reference to the researches of Caigniard de la Tour and Andrews upon the physical properties of gases, wherein it is shown that the laws in which Boyle, Marriott, and former experimenters enunciated the results of apparently complete investigations into the same subject were really only approximations to a solution of the problem. More recently Mendelejeff has shown, by very refined and laborious experiments, that Boyle's law is not strictly true, and he has paved the way for a higher generalisation. But space forbids me to enter into these details.

We generally regard a well established physical law as acting continuously throughout all past time. Of course this is merely an assumption, yet it is an assumption which is apparently necessary in most cases if we are to attempt a scientific solution of the problems of the Universe. But there are good reasons for believing that certain very well

established generalisations of science may not have held good during all past time. Sir Wm. Thomson has shown how to deduce (from Fourier's "Theorem of Heat") in certain cases the thermal state of a body in past time from its known condition at present, and one of the results of his investigation is the indication of "A certain date in past time such that the present state of things cannot be deduced from any distribution of temperature occurring previously to that date, and becoming diffused by ordinary conduction. Some other event beyond ordinary conduction must have occurred since that date in order to produce the present state of things. This is only one of the cases in which a consideration of the dissipation of energy leads to the determination of a superior limit to the antiquity of the observed order of things."*

It is possible to imagine a law which should exhibit a break, or breaks, of continuity. Babbage has shown that it is theoretically possible to devise a machine which shall work according to a fixed law for any finite period of time, and yet at a fixed moment exhibit a single breach of the law. The machine might, for instance, be constituted so as to continue counting the natural numbers for an immense period of time. "If every letter in the volume now before the reader's eyes," says Babbage, "were changed into a figure, and if all the figures contained in a thousand such volumes were arranged in order, the whole together would yet fall far short of the vast induction the observer would have had in favour of the truth of the law of natural numbers. . . . Yet shall the engine, true to the prediction of its inventor, after the lapse of myriads of ages, fulfil its task, and give that one, the first and only exception, to the time-sanctioned law. What would have been the chances against the appearance of the excepted case immediately prior to its occurrence?"†

In the application of scientific generalisations we assume that the future will be as the present; we overlook, necessarily, the chance of sudden interferences with the present order of things. Yet we have no ground for denying the possibility of such interferences. There are facts which make the existence of numerous dark bodies in space very probable. How do we know that by the collision of one of these unseen bodies with this planet the present order of things may not be suddenly terminated? Have we investi-

* Clerk Maxwell, Theory of Heat, p. 244, 245.

† Ninth Bridgewater Treatise, p. 140, quoted by Jevons, Principles of Science, vol. ii., p. 437.

gated all the hidden springs of energy within the earth itself? Is there no chance of a sudden outbreak of some kind which will destroy this world and all its inhabitants in the twinkling of an eye? These suppositions are not unscientific: it is unscientific to assume complete knowledge when we really know almost nothing.

I think I have said enough to show that the scientific method is necessarily limited; that it leads us to recognise our own ignorance and the vastness of the problems presented for solution.

In an early part of this paper I said that by the aid of science we rise from the changing to the changeless; but if what I have said concerning the limitations of scientific laws, and concerning the unknown possibilities of Nature be true, it would appear as if the firm standing-ground we had seemed to gain were vanishing from under our feet. In a sense it is so; in another, and higher sense, the ground remains sure and firm. Science, when we know our littleness and the greatness of Nature, exhibits to us the reign of law, but bids us beware of placing our partial interpretations upon her laws; she commands us to proceed in the investigation of facts, but to be very careful how we interpret these facts. We have learned enough already to lead us to believe that although we can never fathom the mysteries of the Universe, the Universe is nevertheless obedient to order. If that little portion of the Universe which Science has conquered to herself be so wonderful in its organisation and in its working what must the whole Universe be?

IV. CORNISH CHINA CLAY.*

By James Quick.

THE art of pottery being one of those branches of industry which supply the immediate wants of the human race, originated in the earliest period of mankind's existence, and at the present day no other of the useful arts has attained a higher degree of excellence in its various details or has such a universal demand for its numerous products. In discovering suitable materials for the pottery manufacture, as well as in perfecting the different processes of the industry, an extraordinary amount of talent in every age and in almost every nation has been expended, and for the antiquary and the historian the subject as a whole presents an inexhaustible mine of research. The story of Bernard Palissy in France, wasting his time, his energy, and his money in his efforts to solve the mystery of the white enamel, and that of Baron Böttcher—the chemist in Saxony whose unremitting perseverance through a long period of years was at last crowned with success through the merest accident—are vested with thrilling interest. The porcelain or white earthenware products of China and Japan, where the manufacture was probably started many years previous to the Christian era, had on account of their pure whiteness, their semi-transparent texture, and superiority of glaze, long been the envy of European artists, when Böttcher one day inspecting a white substance found in Saxony and newly introduced as hair-powder, was struck with its adaptability to the requirements of pottery, and afterwards identified it as being the much-coveted kaolin, or the same material as that used by the Chinese. Subsequently, kaolin in various degrees of purity was found in several other parts of Europe, and it is now known to consist of the felspar of white granite in a state of decomposition, and after passing through the raising process somewhat resembles ordinary whiting, but has a much greater density. The kaolin, or as it is more commonly called in England, "china clay," and sometimes porcelain clay, used at the Staffordshire

* In presenting this article for publication the writer wishes to express his sincere thanks to T. H. Stocker, Esq., of St. Austell; Thos. Kinsman, Esq., of St. Austell and Vounder; the Rev. C. M. E. Collins, of Trewardale, Bodmin; and those other gentlemen in Cornwall who have kindly afforded him facilities for obtaining information.

Potteries, and which is the chief constituent of all the porcelain or finer sorts of earthenware, is obtained wholly from the county of Cornwall and the adjoining district of Dartmoor. It is the purest or most highly esteemed description of the clay known, and may be called the mainstay of our English manufacture, without which we could not possibly hold our pre-eminence over other nations in the excellence of our products, and in a commercial point of view the china clay trade of Cornwall has a much greater importance than has hitherto generally been supposed.

The application of Cornish china clay to the purposes of pottery is due to the enterprising spirit of William Cookworthy, of Plymouth, whose persevering efforts in the cause of the ceramic art are only surpassed by those of Josiah Wedgwood, the "founder," or rather great improver, of the Staffordshire potteries. Mr. Cookworthy opened a pottery at Plymouth in 1733,* and probably used at first only the inferior clays of Dorset and Devon. Owing, however, to the meagre information that is now extant about Cookworthy's labours, the date when he discovered the china clay, or first applied it to any practical use, cannot be accurately ascertained, but from his travels through Cornwall as a chemist, and his peculiar aptitude for geological research, its existence is likely to have been known to him long before he thought of turning it to account. Possibly, too, he may have used the clay for some time privately, reserving his knowledge from the public until some favourable opportunity occurred of divulging it, in accordance with that jealous spirit of secrecy so characteristic of discoverers in the eighteenth century. It appears to have been about the year 1755 that he found a stone at St. Stephen's, Cornwall, which he proved to be identical with the Chinese *petuntz*, or as it is now called china stone,† used for forming the glaze on porcelain, and which is the felspar of granite in a less advanced stage of disintegration than when considered as "clay." From a short account of the life of Cookworthy, published by his grandson,‡ it appears that "he first found china clay and stone at Tregoning Hill, then in the parish of St. Stephen's, and afterwards in the domain of Boconnoc, the family seat of Thomas Pitt, nephew of the Earl of Chatham, and afterwards Lord Camelford." In 1768 Cookworthy, in company

* See a small pamphlet entitled Relics of William Cookworthy, by JOHN PRIDEAUX. London: Whitaker and Co., 1853.

† This was the first time that china stone was found in Europe.

‡ Memorials of Wm. Cookworthy by his Grandson, with an Appendix, London: 1854. Also another Appendix published in 1872.

with Lord Camelford and others, secured by patent* the exclusive use of the Cornish china clay and stone, and with these materials carried on the porcelain manufacture for five or six years at Plymouth, that town, of course, being able thereby to boast that it is the place in England where porcelain was first made. The adventure, however, ultimately proved a commercial failure, and in 1774 Cookworthy sold his patent to Richard Champion, a merchant of Bristol, and his works were transferred to that city. Soon afterwards Champion, in conjunction with other eminent potters, removed the undertaking to Tunstall in the Potteries; and thus raised to the zenith of its completeness our present wonderful and extensive British earthenware industry.† Cookworthy is said to have made several improvements in the details of the porcelain manufacture, and old Plymouth china is now held in high esteem by connoisseurs.

Of late years, also, the Cornish china clay has been applied to a variety of purposes besides that of making porcelain, and doubtless ere long many more channels of usefulness may open up for it, and Cookworthy's discovery even yet attain a still greater importance among our English commercial products. Indeed it is calculated that only about one-third of the quantity now annually raised is employed in the home manufacture of porcelain, and more than another third of the total quantity is exported to foreign countries. The clay, on account of its bleaching properties, is used for "sizing" cotton goods in the Lancashire and Cheshire districts, and also very largely among paper-makers. It is also used in France in the preparation of ultramarine, and in Germany in connection with the manufacture of gilt mouldings. It has also been alleged that on the Continent the kaolin is widely used for adulterating flour; although, if this be true, for the comfort of consumers it is well to know that the clay is perfectly harmless when taken in small quantities.

The district of Cornwall in which the largest deposits of china clay have been found is in the neighbourhood of St. Austell, in the parishes of St. Austell, St. Mewan, St. Stephens, St. Dennis, St. Enoder, Roche, and St. Blazey, and there are deposits on a smaller scale in the eastern part

* It may be remarked that Wedgwood violently opposed this patent, as he himself, notwithstanding his many improvements in pottery, would never entertain the idea of one for his own discoveries.

† Pottery had been made in Staffordshire many years previous to this, and Wedgwood had commenced his improvements about 1760, but only used inferior clays, until Champion introduced the Cornish kaolin to the Potteries. Coarse butter-pots were made at Burslem so early as 1650.

of the county, at Blisland and St. Breward, near Bodmin, and in the west near Helston. Lee Moor, a part of Dartmoor near Plymouth, also yields considerable quantities of the clay; but as the largest works there are held by merchants who are large raisers in the St. Austell district, it can hardly be said that the Lee Moor enterprise enters into any competition with the Cornish trade.

The natural state in which the clay is found cannot be better described than by the following extract from an able paper on "The China Clay and China Stone of Devon and Cornwall," read before the Society of Arts in May, 1876,* by J. H. Collins, F.G.S., a gentleman intimately connected with the subject:—

"In each of the granite masses which rise like islands in the sea of clay slate forming the western extremity of England, some portions have their felspar so decomposed as to be converted into kaolin, or china clay. Other portions are less decomposed, and of somewhat different composition, and these supply the china stone. These decomposed portions are always associated with veins of black tourmaline and other minerals containing fluorine. The mode of alteration of the granite rocks in these neighbourhoods, in my opinion, has certainly been effected by fluorine and other substances coming up from below, and not by carbonic acid and water acting from above. As a rule, the decomposition is more general near the junctions of the granite with the surrounding rocks than elsewhere, but to this rule there are some notable exceptions.

"The natural clay rock is almost always covered with a thick layer of stones, sand, or impure and discoloured clay, known as 'overburden.' This capping often much resembles glacial drift, but it never contains any scratched or glaciated stones or travelled blocks. This capping varies from 3 feet to 40 feet in thickness, and it must, of course, be removed before the clay can be wrought.

"The decomposed granite is found at all elevations except the very highest points of the districts, which are always composed of hard rocks; but yet their situation is usually indicated to the practised eye by a peculiar depression of the surface. These depressions are not observed in the case of china stone. The natural clay rock, being a decomposed granite, consists of kaolin, irregular crystals of quartz, and flakes of mica, with sometimes a little schorl and undecomposed felspar."

* A short and eminently practical work, duly reported in the Society of Arts Journal for May 5, 1876.

XUM

No generally accepted conclusion has yet been arrived at as to the direct causes of formation of the kaolin, or china clay. Mr. Collins is of opinion that it is formed through the action of fluorine, because fissures which invariably contain minerals (tourmaline and mica) of which fluorine is an essential ingredient are always present in the clay. Tin, too, is frequently found in connection with kaolin, which some geologists believe is brought there by the agency of fluorine. To this theory of formation we are strongly disposed to incline. Mr. Collins has himself acted upon granite with dilute hydrofluoric acid, and, without otherwise altering its appearance changed it into kaolin. It may be mentioned that most of the kaolin found in Europe consists, with slight variations, of $Al_2O_3,2SiO_2+2H_2O$.*

The other principal materials used in the preparation of porcelain are bones and flint, the latter being employed for hardening the ware.† Inferior descriptions of clay used in the Potteries are obtained from the counties of Devon and Dorset, especially from the neighbourhod of Teignmouth, where the much-used ball clay is found. At present there are 117 china clay works in Cornwall leased or rented from the freeholders of the land, and it has been roughly calculated that about 1600 working hands altogether are employed. In Devonshire there are eight different clay works, all in the vicinity of Lee Moor.

The method of raising the china clay in Cornwall is interesting. With the exception of a few modern improvements it is very similar to that pursued for many hundreds of years past by the Chinese clay raisers. And indeed many of the plans in clay raising and pottery manufacture invented and adopted by Europeans are merely unconscious repetitions of those practised long before by the workers in China and Japan. The clay is worked in open cuttings, which at some of the principal works are of very considerable extent and

* No very great amount, indeed, of scientific inquiry has yet been brought to bear upon the subject of the kaolin formation; nor does any chemical work to which we have been able to gain access bestow much attention on the point. WATTS's Dictionary of Chemistry, vol. i., says "It (kaolin) may be supposed to be formed from orthoclase, or $K_2OAl_4O_36SiO_2$, by the abstraction of the whole of potash and $\frac{3}{4}$ silica, and addition of 2 at. water," but offers no suggestion as to how this may by nature be brought about. Watts also says "the averagecomposition (of kaolin as naturally found) is $Al_4O_3,2SiO_2+2Aq$," but Ure's, Tomlinson's, and other scientific cyclopædias agree with the formula $Al_2O_32SiO_2+2Aq$, which latter is the most correct, so far as the *major* quantity of kaolin found is concerned.

† Pegmatite, a substance found rarely, but which deserves more attention from geologists than has hitherto been bestowed upon it, is said to contain in its natural state all the necessary ingredients for porcelain.—See TOMLINSON'S Cyclopædia of Useful Arts.

depth, the latter varying from 30 to 120 feet. Over the sloping sides of these workings streams of water are directed which in their course carry with them the deposit of clay mixed with sand and mica, the proportions varying at the several works, but averaging about 8 tons of sand and mica per ton of china clay. During the progress of the streams down the sides—or, as they are locally called, "stopes"—of the cuttings, shovels are used for causing the separation of the clay and sand, and the several streams meet at bottom, carrying with them the liquid mixture into a reservoir, where the sand and much of the mica becomes deposited. At some works the liquid is then pumped, and at others it flows from the reservoir by natural gravitation into a series of long channels called "micas." These are from 2 to 6 feet wide and from 4 to 9 inches deep, varying with requirements and size of streams to be passed, and in length from 100 to 500 feet. The channels have a slight fall, and have weirs or dams placed about 20 feet apart, which, of course, check the stream at intervals and cause the heavier portions of the mixture to fall to the bottom, and these latter are afterwards run off and re-washed, or sent away as refuse.* Passing from the "micas," the solution, being clay alone, flows into large "tanks" averaging about 40 feet long, 50 feet wide, and 6 feet deep, where it settles, the water running off as the deposit becomes denser and the tank fills. If the clay is to be dried in the open air the thickened clay is then run off from the tanks into "pans," which are excavations of from $1\frac{1}{2}$ to 2 feet deep, of various sizes, and sanded at bottom. When sufficiently solid the clay is cut into square blocks and ranged in rows on the ground, and, being exposed to the influence of sun and wind, soon becomes dry. Of late years, at some works, kilns for drying the clay have been introduced: they vary in length from 100 feet and upwards; the clay is conveyed to them from the tanks by various methods, and their drying capacity differs according to circumstances and requirements.

The arrangements for transporting the clay from one part of the county to another are almost as complete as can be

* A marked feature of the Cornish county, and also of parts of Devon, are the white and red streams of refuse water which respectively are discharged from the clay works, and the iron mines, and flow through the green fields and valleys till they reach the sea, where they often form a serious obstacle to the prosecution of the coast fisheries. The other materials (much of which is used for inferior purposes, such as tiles, chimney-pots, fire-bricks, &c.) dug out whilst excavating china clay are overburden (common earth, rubble, &c.), fine mica, coarse mica, discoloured clay.

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desired. The Cornwall Minerals Railway, running from Par to Newquay, passes through the important clay district of St. Austell, and connects the English and Bristol Channels and the Cornwall Railway, stretching the whole length of the county, has many small branches for conveying mineral and other commodities. It is noticeable that the clay sent to the English Potteries is taken from Cornwall wholly by water, that for Staffordshire being shipped to Runcorn, and that for Worcestershire to Gloucester, from which places the material is carried by canal; and this custom of transporting the clay by shipping probably arose from the strong preference shown by Wedgwood for water-carriage for porcelain rather than carriage by land, and his remarkable efforts in furthering the cause of canal navigation in the neighbourhood of the Potteries. The quantities transported through Runcorn in 1876 were 50,222 tons of china clay and 10,633 tons china stone; those through Gloucester were 2456 tons china clay and stone. The following are the amounts* of clay and stone sent from Cornwall in 1876, the places mentioned being the only ports of shipment. Owing to various causes, to which we shall presently refer, the total quantity is said to be less by many hundreds of tons than the average exports for previous years. From Par 35,000 tons of clay and stone were shipped coastwise and 15,000 tons to foreign countries; from Fowey, 61,070 tons coastwise and 14,760 tons foreign; from Charlestown, 34,016 tons coastwise and foreign; from Pentewan (clay only), 4727 tons coastwise and 8786 tons foreign; from Falmouth, coastwise 15,043 tons and 3395 tons foreign, clay and stone; Newquay, total 7,200 tons coastwise and foreign; from Wadebridge, total 2727 tons; from Plymouth, Sutton Harbour, 2345 tons coastwise and none foreign; and from Plymouth Great Western Dock 28,093 tons; total, 232,160 tons of china clay and stone exported from the various works in 1876. The prices of the clay and stone vary according to the purity of the article and, of

* Kindly supplied by the respective harbour-masters; but owing to the different methods of keeping accounts at the various harbours it is impossible to distinguish in each case the amounts of china clay from china stone, or the quantities sent coastwise from those exported to foreign countries. The clay shipped from the Plymouth Great Western Dock comes wholly from the neighbourhood of Dartmoor; that from Sutton Harbour, Plymouth, comes partly from Dartmoor and partly from Cornwall. It must also be understood that the figures here given do not represent the quantities actually *raised* in 1876. Many tons probably of the quantities given were kept over from 1875. Some interesting particulars about Cornish china clay and stone may be obtained from the "Mineral Statistics," published by Prof. Hunt, of the Museum of Practical Geology, Jermyn Street, W.

course, the many circumstances which influence commercial markets. They at present (July, 1877) range from 15*s.* to 30*s.* f. o. b.

As in excavating and preparing the clay no great amount of skilled workmanship is required, only men of the common labourer class are as a rule employed. These are generally under the superintendence of foremen, or, as they are locally called, "captains," who are men that have raised themselves by ability and experience. The wages of the men are 2*s.* 6*d.* for a day's work of seven and a half hours, a rate of pay which—especially when considering the abundant time afforded for extra work, and the facilities in Cornwall for cheap living among the poorer classes—compares most favourably with the wages earned by labourers of other avocations. The men, too, are not checked for rainy days when they cannot work, and the occupation is a very healthy one. In some parts also a system of piecework is adopted, under which the men usually make from 3*s.* to 3*s.* 6*d.* per day. With these earnings the Cornish labourer—renting perhaps a small respectable house for about £3 or £4 per annum, and, if he be married, his wife and children probably being also employed in the clay works, or in connection with the neighbouring tin or copper mines—may be said to rank in the social scale with skilled artisans in many other parts of England.

Until within the last two or three years the relations between the men and their employers had been noticeably good, but in the fall of 1875 the masters, as the trade was very dull, decided upon reducing the rate of wage by 3*d.* per day. This was strongly and openly objected to by the men, and ultimately the rate was allowed to remain at 2*s.* 6*d.* A Trades' Union was then formed among the labourers, ostensibly for regulating the "rates of wages and the hours of work," although from its subsequent proceedings it would seem rather to have been for annoying the masters as much as possible in revenge for their attempt to shorten pay; and although by no means wishing to depreciate the value of Trades' Unions generally, yet we must certainly say that this short-lived union presents an example of the excess of folly to which well-intentioned but misguided men can be urged by the representations of factious agitators. Notwithstanding, too, that several months have now elapsed since the strike occurred and the animosities excited by it have mostly subsided, yet we think a brief and impartial statement of the facts of the matter here to be not alto-

gether inappropriate, even if it only serves to exhibit the sturdy independence and almost stubborn fixidity, when once a purpose is resolved upon, of the Cornish character, glossed over, as that character is, by a spirit of extreme enthusiasm for whatever is undertaken. For our friends on the western side of the Tamar, in spite of the extinction of their language and the gradual decay of Cornish superstitions and prejudices, have still some remarkable traits of disposition quite as distinctive as those of any native of the Land o' Cakes or the Emerald Isle. Doubtless had the masters, during the time of the strike in question, been other than Cornish ones, the men would have obtained compliance with their demands long before they carried their actions to such an excess of riot as was reached; and had the labourers been other than Cornishmen, affairs, under the determined measures of the masters, would have assumed a pacific appearance much sooner than they did. Emboldened by the "big talk" of those persons who preferred hearing the sound of their own voices to performing honest labour, the members of the Union, towards the close of 1876, arranged to strike work on a certain day, and it was also agreed that those non-unionists who determined to work on that day should be forcibly prevented from so doing. Notices to this effect were posted up in the various towns in the St. Austell district, and accordingly, on the day in question, the unionists refused to work, and some most disgraceful scenes occurred.* The non-union men had, in the words of a local paper, to "cut and run," many of them having to hide for hours from the vengeance of the unionists. The men on strike visited the various works where the non-unionists were employed, obliging the latter to discontinue their occupation. This was done for several days, and although few deeds of violence were actually perpetrated, threats were very freely used, and the non-union men were in a state of great alarm. On the ringleaders of the strike being apprehended, one of them was fined £5 and costs for

* The following extract from a letter which appeared in the "Plymouth Daily Mercury" during the time of the strike above described, may serve as an instance of the excess of violence to which the Cornish working classes can allow themselves to be driven. It refers to a strike among the tin and copper miners some years previous. During the strike in East Cornwall it was "considered a trivial matter to place a poor fellow across a pole, and to shoulder him through the village of Gunnislake until the life was nearly jolted out of his body, to the delight of the leaders of the strike. The magistrates of Callington, however, took a different view of the matter, and Bodmin Gaol had in consequence some extra occupants for a considerable period after the strike had died out."

pointing out a certain individual for some other man to obstruct, another was sentenced to five weeks' hard labour for breaking the collar-bone of a non-unionist, and the president of the union got ten weeks' hard labour for going about encouraging the crowd in their acts of intimidation. The masters stated their determination to employ whom they pleased, and they preferred to employ only non-union workers; and this they did. Many of the malcontents returned to work at the old rate of wages, but a large proportion emigrated to Australia and other places.

During the last three or four years the Cornish clay trade has experienced a state of almost unprecedented depression, and, owing to the many new sources of clay supply which have of late years sprung up in various parts of Europe and elsewhere, grave doubts are entertained by some as to the future prospects of the Cornish trade.* Extensive works have been opened in Allier, Finisterre, and other departments in the north of France, and these now supply many of the French potteries where formerly Cornish clay was used. The clay districts of the eastern parts of the United States have also been more largely worked of late, the demand for the Cornish material in that country being thereby greatly diminished. The kaolin deposits, too, in the island of Bornholm now supply largely many of the paper-mills in Sweden, Denmark, and North Germany, and lessen by many hundreds of tons the annual exports from the Cornish districts. These misgivings as to the state of trade, however, do not appear to be shared by the majority of Cornish clay merchants, and an impartial consideration of the facts of the case must, in our opinion, lead to the conclusion that there is—at least as yet—very little ground for apprehending any permanent depression. We think the present unsatisfactory condition of affairs is owing chiefly to the unusual but now long-standing dulness of trade generally, and especially of the pottery trade and other departments of commercial enterprise in which the clay is employed; and when these branches of industry shall have revived, as they already are beginning to do, the Cornish clay trade will doubtless gradually recover itself. Another special cause of the present depression of the Cornish trade was over-speculating, in the years 1872-3. In those years many new clay-works were opened up by persons or

* See letter in the "Plymouth Mercury" of January 17th, 1877.

companies who did not understand the nature and proper modes of working china-clay works, or who were ignorant as to what could and what could not be worked at a profit. The result with such people is, that after large expenditure they find themselves overstocked with china clay, raised at heavy cost, and for which they cannot find a sale at remunerative prices; and their clay is now being disposed of for what it will fetch, some even being sold by auction at a merely nominal value, either to wind up the concerns, if they be public companies, or to get back money for pressing emergencies to those who have not sufficient capital to withstand the fluctuations of the market. At present, too, the habit of "forcing the royalties" forms a serious obstacle to the progress of the clay trade. The royalty is a certain sum per ton payable to the landowners by the lessees on all clay raised. The lessees or renters of the land generally pay a fixed minimum rent, which is allowed to merge into the royalty until the royalty exceeds the minimum rent, when the former is paid no matter what amount it may reach. Some of the lessees who have very large works, and whose leases are soon to expire, have been forcing their outputs of clay, and selling at a very reduced rate, in order to gain favour with their landlords by raising the royalties to as large a sum as possible beyond the minimum rent so as to obtain new leases on favourable terms. This method of proceeding is a well-known fact only to the few concerned, but the wisdom of a course which it is plain must seriously damage the market is certainly very questionable. The decrease in the demand for the Cornish article, owing to the more extensive use of native clays in America and other parts, will probably be now soon counterbalanced by the increased development in those branches of industry in other places where the home clay is employed. And even if there should be some cause for alarm it ought only to stimulate the Cornish proprietors to discover fresh means of disposal for their material. The scheme suggested a few years since for establishing a local industry by the introduction of the manufacture of porcelain *in situ* or into Cornwall instead of

* The fixed royalty dues vary from 2s. 6d. to 4s. per ton. The system of royalties may perhaps be made clearer from the following illustration:—Suppose the minimum rent is £100, and the dues 4s., the tenant must raise 500 tons of clay (at 4s. per ton) to cover his rent. Whatever quantity under 500 tons is raised he *must* pay £100; but whatever quantity *over* 500 tons is raised he would pay 4s. per ton—thus 1000 tons, say at 4s., £200—hence the rent is said to merge in the dues or royalty.

sending the raw commodity to the Potteries deserves to be more attentively considered than it has been hitherto. This scheme was originally brought forward by the Rev. C. M. E. Collins, of Trewardale, Bodmin, in the year 1868, in an able lecture on the subject delivered by that gentleman before the Royal Polytechnic Society of Cornwall (and duly reported in the Society's Journal, October, 1868), in which the feasibility of such an undertaking was fully demonstrated. The crucial difficulty urged against the Rev. Mr. Collins's proposals is that of the want of coal, as all coal, of course, used in the county has to be carried there from other parts. But Mr. Collins has shown that the amounts now expended annually in sending the clay to the potteries, and again in carrying the manufactured articles from the potteries to Liverpool for export, and in keeping agents in the potteries by the Cornish firms, far exceeds in any average period the outlay that would be entailed in the single article of coal. Indeed, we think numerous advantages would, on the whole, result from the adoption of Mr. Collins's plans, not only to those immediately connected with the Cornish clay trade, but also to the public generally; for Cornwall seems eminently fitted to be the seat of a pottery manufacture, all the porcelain now made at the potteries not only having to leave the county in the shape of the raw material, but also mostly having to pass her shores when being exported to foreign countries, and she has harbours well suited for accommodating any vessels that might be engaged in the carrying trade, and a large amount of sea freight and risk could be saved. By the new enterprise fresh improvements, too, in machinery and other matters could be introduced, which either prejudice or an unwillingness to incur expense prevents many of the present owners of the potteries from adopting, and the final result would doubtless be that an even better description of pottery than that now made could be produced at a much lower price. Mr. Collins calculates that to start his scheme in full working order would require about £30,000.

Already inferior articles, such as fire-bricks, tiles, &c., are manufactured in Cornwall, and an earth is found in the county well adapted for making the *seggars* or ovens in which the porcelain is baked. Of late, also, articles of an ornamental as well as useful character have been manufactured from china clay and stone, both in their raw state (or without undergoing any heating process), and after being made into porcelain paste, and there seems to be every likelihood that the clay may be still more extensively employed for the

purposes of decorative art. Statues, mortuary tablets, ornamental chimney-pieces, dressing-tables, blocks for church-pillars, and many other articles, are now made from the clay or stone, and some Mosaic floor-work* in china clay, recently manufactured, is considered to rival in appearance Devonshire alabaster.

All things being taken into account, we are of opinion that if the Cornish clay merchants continue to manifest that sage firmness of conduct which has characterised them under recent difficulties, their trade will soon emerge from its present depressing crisis and regain its former flourishing condition, and we strongly urge clay proprieters and all other persons interested in the welfare of the manufacturing arts, and desirous of profitable investment, to bestow their gravest attention on the scheme we have just discussed, by the adoption of which the clay business as a whole would, beyond a doubt, reach an even higher degree of prosperity than it has ever before attained.

V. THE SIGNIFICANCE OF THE PHENOMENA OF ONTOGENESIS IN REFERENCE TO THE EVOLUTION HYPOTHESIS.

By J. HUDDART.

TO afford a due appreciation of the importance of the relation of individual to racial development it will be necessary to review briefly some of the leading opinions on the Theory of Evolution, and to observe their tendency.

While most of our biologists and physicists agree that no better explanation can be advanced for observed facts than that all existing and extinct species of animals and plants have been evolved from one or more primordial forms, they are by no means in accord as to the agent or agents that have effected this evolution.

The professors of what is commonly known as the Darwinian Theory, but what might be more properly termed

* Specimens of this description of work in china clay may now be seen in the South Kensington Museum.

the Doctrine of Descent, may be broadly divided into two classes—one of which comprises those who claim to be the exponents of the Darwinian view of Evolution, and who regard all structural changes as due to natural and sexual selection and other casual causes; and, in general, consider all organic processes, development, growth, and the modification of species to be the result merely of a very refined and complex action of the molecular forces.

Oscar Schmidt, who is one of this class, states his position very clearly in the "Doctrine of Descent and Darwinism." He remarks that the physical view in its perfection reduces every organic process to a problem of pure mechanics, and quoting A. Fick—"I am of opinion that the mechanical view of organic life is demonstrated only when all the motions in an organism are shown to be the effects of forces, which at other times also are inherent in its atoms. But, similarly, I should regard the vitalistic view as proved if in any case a particular motion actually observed to take place in an organism were shown to be mechanically impossible. . . . . I provisionally profess myself unequivocally in favour of the mechanical view." I was surprised to find in this work of Oscar Schmidt's a quotation from the "Origin of Species," in many points resembling the concluding remarks in a paper of mine contributed to the last January number of this Journal. Although I do not remember having read the passage in the "Origin of Species" prior to writing the paper I refer to, I think I must have been unconsciously influenced in some way by the memory of Darwin's eloquent words, which are as follow:—"There is grandeur in this view of life, with its several powers, having been originally breathed by the Creator into a few forms or into one, and that whilst this planet has gone cycling on according to the fixed law of gravity, from so simple a beginning endless forms, most beautiful and most wonderful, have been and are being evolved." "In this concession," Oscar Schmidt remarks, "Darwin has certainly been untrue to himself [*or to his followers' interpretation of his theory ?*], and it satisfies neither those who believe in a personal God nor the partisans of natural evolution. It is directly incompatible with the doctrine of descent [*according to the physical view*], or as Zöllner says:—'The hypothesis of an act of creation (for the beginning of life) would not be a logical, but a merely arbitrary, limitation of the causal series.'" Again, he remarks:—"To anyone who holds open the possibility that even now animate may be evolved from inanimate existence without the mediation

of progenitors, the first origin of life in this natural method is at once self-evident. But even if the proof were given, which never can be given, that in the present world spontaneous generation does not occur, the inference would be false that it never did." It is evident from these remarks that Oscar Schmidt is fully sensible of the important bearing of the question of spontaneous generation on the mechanical theory of life, a subject we shall discuss later in this paper.

The mechanical view of evolution is held by Professors Huxley and Tyndall, and by Mr. Herbert Spencer and others.

We have referred to another class of evolutionists. It is represented by those who, while admitting the sufficiency of natural selection and other extraneous causes to account for a wide range of facts, do not consider that they are adequate to explain *all* the phenomena connected with animate nature. Apart from this general agreement their views do not altogether coincide. The following short descriptions of some of the most important opinions will greatly facilitate the reader in forming an estimate of the value of the conclusions hereinafter arrived at, as it will be seen that these opinions (notably that of Mr. Wallace), derived from general considerations, coincide in a remarkable manner with the results deduced in this paper from independent sources.

As is well known, Mr. Wallace, independently of Mr. Darwin, elaborated the theory of natural selection, but he differs from Mr. Darwin in some important particulars. He does not consider natural selection adequate to account for the cerebral development of a savage, who, while possessing a brain little inferior in size to that of a philosopher, has mental qualities very slightly superior to those of some of the higher mammals. Thus the enlargement of the brain, which is equivalent to an increase of mental capability, is unnecessary to the savage and in advance of his requirements, and is therefore due to some cause other than natural selection. A reserve of mental capacity has been accumulated in the savage, which renders him capable of rapid civilisation and improvement. Natural selection fails also to account for the absence of hair on the human body, as its loss could in no wise have been beneficial [*Darwin, however, considers this to be due to sexual selection*]. The extreme development of the larynx in the female sex could never have been acquired by savages, as it is beyond their wants, and their known habits could not have effected it. From these and other considerations, such as the want of relation between a number of man's mental qualities and his material

progress, Mr. Wallace makes the following very remarkable deductions;—"That a Superior Intelligence has guided the development of man in a definite direction. . . . Even if my particular view be not the true one, the difficulties I have just put forward remain, and I think prove that *some general and more fundamental law underlies that of natural selection.* . . . It is probable that the true law lies too deep for us to discover it; but *there seems to me to be ample indication that such a law does exist, and is probably connected with the absolute origin of life and of organisation.*" In a note to this paragraph he says that some of his critics have accused him of believing that "our brains are made by God and our lungs by natural selection." This would be a legitimate objection if true, for if there be a law underlying that of natural selection it is universal; we have but to search for its workings, to trace its operations, and we shall find them throughout organic nature; not only in the human cerebrum, but in every part of every living thing, both animal and vegetal. This is, I think, also Mr. Wallace's belief.

Mr. St. George Mivart believes in an internal tendency to perfection. In his "Genesis of Species" the following remarks occur:—"Now it is here contended that the relationships borne one to another by various component parts imply the existence of some innate internal condition, conveniently spoken of as a power or tendency, which is quite as mysterious as is any innate condition, power, or tendency resulting in the orderly evolution of successive specific manifestations. These relationships, as also this developmental power, will doubtless in a certain sense be somewhat further explained as science advances."

Even Mr. Darwin, with characteristic candour, writes:—"I now admit, after reading the essay by Nägeli on plants, and the remarks by various authors with respect to animals, more especially those recently made by Professor Broca, that in the earlier editions of my "Origin of Species" I probably attributed too much to the action of natural selection, and the survival of the fittest. I have altered the fifth edition of the 'Origin' so as to confine my remarks to *adaptive changes of structure.*"

Professor Owen, while acquiescing in the doctrine of descent, does not accept the theory of natural selection *in toto.*

Thus there seems to exist a tolerably general sense of insufficiency in the theory of selection, and the tendency is, in a great measure, to return to views approaching those of

the great French naturalist who first systematised the evolution hypothesis, crudely and imperfectly it is true, but with what breadth and grasp of generalities! Lamarck, perhaps, attained nearer to the truth than some of those to whom the glory belongs of having in late years elaborated the great theory of Natural Selection, and of having revived interest in a subject, the importance of which cannot be over-estimated. He believed in a primary developing cause, of the nature of which he had no conception, and in a form of spontaneous generation, because he could not otherwise account for the beginning of life: to quote his own words—"That all the organisms of the world are the true productions of Nature, and that they have been very gradually evolved. That Nature, in her course, commences and ever re-commences, by forming the simplest organisms, and that she only directly forms these, that is, the first germs of organisation, which have been designated 'spontaneous generations.'" He also maintains that Nature has implanted in the first germs of life a faculty of growth. It would appear that he does not intend to identify the agent he calls Nature with mechanical causes. The following quotation from the Introduction to his "Philosophie Zoologique" seems, however, to point to a contrary conclusion:—"That it is, unquestionably, a very great and profound truth, that all operations, moral and physical, are due to the same source. . . . And, in short, that all the phenomena of the intelligence and the will have their origin in the primitive and accidental state of the organism." Although not very clear as to the cause he has no doubt as to the fact of a progressive development having taken place since the commencement of life in the world: "a fact," he says, "which deserves all the attention of those who study the nature of animals; a fact partially seen many centuries ago, never completely grasped, always exaggerated." He then proceeds to point out that the reality of the progression does not need to be established by an act of reasoning, but is simply an observed phenomenon. He strongly repudiates the opinion, which he complains had been attributed to him, that animal and vegetable life had a common origin, and he also declares his belief that living cannot have sprung from not-living matter. His conception of spontaneous generation must therefore have differed considerably from modern notions on the subject. He remarks that the primary cause has been disturbed here and there in its action by a casual, and consequently varying, secondary and modifying cause.

As yet, as far as I can discover, no evolutionist has pointed out the full significance of embryological phenomena in reference to the Evolution hypothesis.

The analogy between the development of the individual and the unfolding of the primordial germ has been regarded hitherto only as a corroboration of the doctrine of Descent.

All organisms commence their existence, *de novo*, from the lowest types. Instead of at once developing the parental characteristics they pass through metamorphoses similar to those which have, in past ages, succeeded each other in the progressive development of their families.

This very striking parallelism of phenomena suggests, I think, the true solution of the mystery of Evolution.

The fact of the orderly reproduction of ancestral forms in the phases of individual development is so general that there can be no question as to its being a fundamental law of Nature. In order to illustrate its universality, I have chosen a few extracts from Dr. Carpenter's "Human Physiology," and from the work of Oscar Schmidt already referred to.

The first condition of the primordial germ is common to all animals, and is permanent in the lowest forms: there is no indication at this stage to what form of organic structure it may ultimately attain. Development commences by duplicative subdivision; a mass of cell-like bodies is thus produced whose component parts are all alike. This phase also has its parallel among the simpler organisms (the *Hydra* and *Planaria*). In all vertebrate animals the appearance of the primitive trace indicates the primary division to which they belong: this trace represents the vertebral column, and is first laid out as an unsegmented cord and an unsegmented sheath for the spinal cord; this is the permanent state of the lower fishes. In the development of the Vertebrata the vitelline vessels correspond to the mesenteric veins of invertebrated animals, and the blastodermic vesicle is to be regarded as the temporary stomach of the embryo, remaining as the permanent stomach in the radiated tribes. The circulation of Mammalia is at first carried on exactly on the plan which is characteristic of the circulating apparatus of fishes. The aorta subdivides on either side of the neck into three or four arches, which are separated by fissures much resembling those forming the entrances to the gill-cavities of cartilaginous fishes; and these arches re-unite to form the descending aorta, which transmits branches to all parts of the body. In the higher Vertebrata, however, the plan of the circulation is afterwards entirely changed by the

formation of new cavities in the heart, and by the production of new vessels. The clefts between the branchial arches close up and disappear. In the amphibians the branchial arches really bear gills during the larval state. A temporary respiratory apparatus, the allantois, bearing a strong resemblance in its own character, and especially in its vascular connections, to the gills of the Mollusca, is developed at an early stage: it seldom attains any size in Mammalia, being early superseded by another provision for the aëration of the blood. Many of the malformations occasionally observed in man are for the most part due to arrest of development; the circulating apparatus is sometimes permanently fixed in conditions that are properly characteristic of cold-blooded animals: and it is interesting to remark, too, that the varieties which not unfrequently present themselves, in the arrangements of the principal trunks given off from the aorta, find their analogues in the arrangements that are normally characteristic of some one or other of the Mammalia. The venous system of the human body undergoes changes which are even more remarkable than those of the arterial trunks. In its earliest condition it has been ascertained by Rathke to present essentially the same type in the embryos of all vertebrated animals, the peculiarities of each group being acquired by a process of subsequent transformation. There is at first a pair of anterior venous trunks, receiving the blood from the head, and a pair of posterior trunks, formed by the confluence of the veins of the trunks, of the Wolffian bodies, &c.: the former are persistent as the jugular veins; the latter remain separate in most fishes, where they are designated the Cardinal Veins; but in man (as in warm-blooded Vertebrata generally) they are only represented by the venæ azygos, major and minor, which coalesce into a common trunk for a considerable part of their length. One of the anterior trunks and one of the posterior unite on either side to form a canal, which is known as the Ductus Cuvieri; and the ducts of the two sides coalesce to form a shorter main canal, which enters the auricle, at that time an undivided cavity. This common canal is absorbed into the auricle at an early period in all Vertebrata above fishes; and after the septum auriculorum is formed, the two Cuvierian ducts separately enter the right auricle. This arrangement is persistent in birds and the inferior mammals, in which we find two venæ lavæ superiores entering the right auricle separately; but in the higher Mammalia and in man the left duct is obliterated, and the right alone remains as the single vena lava superior,

a transverse communicating branch being formed, to bring to it the blood of the left side. The double vena lava sometimes presents itself as a monstrosity in the human subject; some vestiges of the original arrangement may be traced even in the normal condition of the venous system in the adult. The embryological development of the urinary organs in vertebrated animals is a subject of peculiar interest, owing to the correspondence which may be traced between the transitory forms they present in the higher classes and their permanent conditions in the lower. Vessels exactly analogous to the corpora malpighiana of the true kidney remain as the permanent urinary organs of fishes, but in the higher Vertebrata they give place to the true kidneys. At the end of the third month (in the human subject) the kidneys consist of seven or eight lobes; their excretory ducts still terminate in the canal which receives those of the Wolffian bodies and of the Fallopian tubes; and this opens, with the rectum, into a sort of cloaca, analogous to that which is permanent in the oviparous Vertebrata. In the embryo the distinction of sexes is altogether wanting at first, and the conformation of the external parts of the apparatus is originally the same in man and the higher Mammalia as it permanently is in the oviparous Vertebrata. A partial representation of the phase of development at the tenth or eleventh week in the human subject is found in the permanent condition of the Struthious birds and of the implacental Mammalia. In the small proportion which the cerebral hemispheres bear to the other parts, in the absence of convolutions, in the deficiency of commissures, and in the general simplicity of structure of the whole, there is a certain correspondence between the brain of the human embryo at about the sixth week and that of a fish. About the twelfth week we find the cerebral hemispheres much increased in size, and arching back over the thalami and corpora quadrigemina; still, however, they are destitute of convolutions, and are imperfectly connected by commissures, and there is a large cavity yet existing in the corpora quadrigemina which freely communicates with the third ventricle. In all these particulars there is a strong analogy between the conditions of the brain of the human embryo at this period and that of the bird. Up to the end of the third month the cerebral hemispheres present only rudiments of anterior lobes, and they do not pass beyond that grade of development which is permanently characteristic of the marsupial Mammalia, the thalami being still incompletely covered in by them. During the fourth and

part of the fifth months, however, the middle lobes are developed from their posterior aspect, and cover the corpora quadrigemina; and the posterior lobes, of which there was no previous rudiment, subsequently begin to sprout from the back to the middle lobes, remaining separated from them, however, by a distinct furrow, even in the brain of the mature fœtus, and sometimes in that of older persons. In these and other particulars there is a very close correspondence between the progressive stages of development of the human cerebrum and those which we encounter in the ascending series of Mammalia. The phases of evolution of all parts of the higher organisms are represented by analogous and permanent conditions among the lower. In the organs of smell, for instance, the several steps of development are met with in the various classes of animals: the small closed fossæ remind us of fishes; the short nasal ducts opening into the anterior part of the mouth, of batrachians. A fine wool-like hair covers the human fœtus during the sixth month: the whole surface, including even the forehead and ears, is thus thickly clothed; but it is a significant fact that the palms of the hands and the soles of the feet are quite naked, like the inferior surfaces of all four extremities in most of the lower animals. As this can hardly be an accidental coincidence, the woolly covering of the fœtus probably represents the first permanent coat of hair in those mammals which are born hairy. The abnormal growths of hair occasionally observed in the human subject are similar in texture to the lanugo of the fœtus, and must be attributed to an arrest of development of the hair together with its continued growth.*

It is a noteworthy fact that, in cases of arrested development, the "blighted" part continues to grow independently of its stage of development.

The higher the organism, as a general rule, the more perfect is the state in which it enters the world: even the new-born human being is, however, far from fully developed; and Oscar Peschel remarks that the course of development of speech in tender years is approximately, if not completely, similar to the first attempts to speak made by our race.

A recent discovery of the Rev. W. Dallinger's has enabled us to trace the parallelism of descent and of individual development even further than from the commencement of embryonic evolution. He has seen a monad with

* Darwin, The Descent of Man.

flagellum (closely resembling a spermatozoon) absorb another monad prior to muliplication. This corresponds exactly with the union of the sperm-cell and the germ-cell in the fecundation of the ovum.*

Having briefly traced some of the reversions to lower types in the higher organisms, I shall now quote a few interesting notes on the individual development of the lower animals and of plants, from Oscar Schmidt's "Doctrine of Descent and Darwinism." He remarks that "The circumstances of parasitic worms are repeated by the parasitic Crustacea, as, moreover, a probably primordial form of the crab family is preserved in the metamorphoses of several orders of this large and diversified, though coherent, class. The larva which, it may safely be assumed, approximates closely to the primordial form, was at one time taken for an independent genus, and received the name of *Nauplius*. Hence a *Nauplius* phase is spoken of which obtains especially among the lower Crustacea, the *Copepoda*, parasitical Crustacea, and *Cirripedes*, and the remarkable *Rhizopoda* connected with them; but is not wanting in the highest order, the decapodous stalk-eyed crab. We shall later have to make acquaintance with the so-called curtailed development which among the crabs has been adopted by the decapods, and it was formerly supposed by all. Were this actually the case we should still, by analogy, infer their connection with the orders repeating the *Nauplius* phase in the course of their development; but it was a welcome discovery of Fritz Müller's that a shrimp (*Peneus*) still begins its development as a *Nauplius*, whereas all the other members of the order, as far as they are known, leave the egg in the higher *Zoea* phase. As of the hundreds of stalk-eyed crabs scarcely a dozen have hitherto been examined as to their development, it will not be doubted that, with regard to the *Nauplius* phase, some resemble the *Peneus* of the Brazilian coast. But even were this case to prove unique in the order, it would suffice as a living witness to the connection between the presence of the decapods and the primordial crabs. There can be no other view of the subject. The *Nauplius* phase in the development of the *Peneus* is either a shining testimony in favour of the doctrine of Descent or a senseless paradox." Again: "The shell of the individual Ammonite begins with the old morphological type, and then adopts the modifications in the same order in which they follow in vast periods in the

* This would seem to indicate that the distinction of sex is coeval with life itself.

geological development of the groups concerned." In another place he says—"The individual development of the *Cladomena*, and other *Medusæ* similarly propagated, corresponds with the systematic series of the *Medusa* polypes." He gives many other instances of parallelism of individual and racial development among the lower forms of life.

But the crowning fact of all is that the vegetable world conforms to the same law. A little further on, in the same chapter, Oscar Schmidt remarks—"Quite recently A. Braun has pointed out the accordance of the botanical system, and therewith of palæontological succession, with the development of the individual plant, when he says:—In the further elaboration of the Natural system, the gradation of the vegetal kingdom, and, at the same time, the relation of the system to the history of development, becomes more and more spontaneously and incontrovertibly manifest. The Acotyledons are verified as Cryptogams, as they were already considered by the old botanists of pre-Linnæan times, and their relation to the Phænogams is thus more clearly pronounced. The Cryptogams are separated into two essentially different divisions, in which gradation is likewise distinctly pronounced (cellular and vascular Cryptogams, Thallophytes, and Kormophytes); between the perfect Phænogams and the Cryptogams an intermediate grade has been shown, that of the Gymnosperms. But most important of all is the circumstance that the four chief grades ascertained in the vegetal kingdom accurately correspond with the grades of development occurring in the individuals of all the higher plants;—the germ, the vegetation stem, the blossom, and the fruit."

The parallelism of the metamorphoses of embryonic development, and the stages of historical evolution, is far more complete than the brief sketch I have just given of its more salient features would seem to convey. Its universality has, however, been sufficiently illustrated.

It is, then, a fundamental law of Nature that the development of each organism, independently, must recommence *ab initio*, and that the phases of the development corrrespond in every way with those of the evolution or expansion of the primordial germ from which it has sprung.

The first and main conclusion to be drawn from this law appears to be that the actual development of organisms is not due to Selection.

The second inference to be derived from it, in connection with other facts, is that organic development is not a mere manifestation of molecular force.

To elucidate the train of reasoning by which these deductions are arrived at, let us, for the sake of simplicity, consider the case of a hen's egg, in which we have two conditions of importance; the one consists in the protection of the embryo by the shell from all external influences: the other in the fact that it is completely isolated from the mother, and therefore that its development cannot be affected by any maternal formative power. Although the *fœtus in utero* is as little influenced by modifying causes, as the chick *in ovo*, the insect undergoing metamorphosis in the pupal stage, or the seed springing from the earth, it is not, in the first case, on a *primâ facie* view, so self-evident as in the latter ones. The conclusions we shall arrive at from the case of the chick will, it is evident, be nevertheless equally applicable by analogy to the whole of the animal and vegetal kingdoms.

When quite newly laid a hen's egg contains no trace whatever of the different systems, organs, and limbs which make their appearance in due order during the period of incubation, prior to which the contents of the egg are in a condition corresponding to protoplasm. Since the albuminous and other materials enclosed in the shell are protected by it from all extraneous modifying influences, the development of the chick (the various phases of which are precisely analogous both in their distinguishing traits, and in the order in which they occur, to those which have succeeded each other as characteristics of its progenitors), is undeniably effected without the aid of selection or any similar agent, but is due to a force within itself. Selection, therefore, is not necessary to development; but a development in every way analogous to that of the race is repeated in each individual by an agency which is not selection. The conclusion seems irresistible that the race was developed by the same agency. The conditions in the two cases are similar, the effects are uniform; are we not, therefore, justified in assuming that the cause in each is identical?

It seems to follow, then, that the phenomena of ontogenesis and those of phylogenesis are manifestations of one and the same developmental law—the law of correlative expansion.

It is worthy of notice that the earlier ancestral specific and generic modifications and adaptations, which undoubtedly are fairly attributable to natural selection, habits, &c., become obliterated and are gradually lost, when the developing agent acts free from extraneous influences, while only the leading types are retained. It is *superfluous* to have recourse to selection to account for racial development, when we know

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that a faculty of evolution exists or can be set up in every organism. Embryonic development is inexplicable on the theory of selection, which cannot account for the recurrence of characteristics of which there is no trace in the parent form, or which belong to totally different species, genera, or families. For instance, it would be idle to ascribe to selection the reversion to the extreme limit of life observed in the spermatozoon. All we could expect in individual development, on the mechanical theory, would be an accurate replica of the parent form without the preliminary grades, that subsequently become effaced, and that have no reference to the advantage of the embryo, but are in general merely reversions to long-extinct progenitors. The developmental power, law, or principle, controlling influence, or whatever it be named, manifests itself in a multitude of organic phenomena. All features in an organism that cannot be attributed to selection must be referred to this influence. Serial and bilateral symmetry and type generally comes under this heading.

A law of correlative development is plainly indicated in the intimate connection and correlation of all parts of organisms, in the fact that no single character can be modified without a corresponding modification of all others; in short, in the bond of union that constitutes all parts of an organism, mathematically speaking, functions of one another and of the whole. This bond of union or correlation explains all cases of rudimentary growths, which must be regarded as necessarily coexistent with certain completely developed organs, limbs, or other portions of the structure, or with a certain stage or form of development of the organism, and not necessarily as parts in a state of transition. The mammæ of males and all secondary sexual characters owe their origin to correlative development or evolution; they are functions of the organs of generation, as it is well known that the male organs of generation are homologous with those of the female. It is impossible to explain this instance of rudiments in any other way, and it must therefore be regarded as a special case affording the strongest possible corroboration of the hypothesis of a developmental law. It is important to note that when the genital organs are abnormally developed and present features peculiar to both sexes in an imperfect condition, the secondary sexual characters also assume an intermediate state.

It is a significant fact, that where it is possible to decide to what progenitor the organism reverts in its embryo, it is generally found to be an extinct one, although closely allied

species are found still in existence. This would appear to indicate that the existing species have been evolved by the same law as the extinct ones, which have in reality become transformed into the higher type.

It is probable that many adaptations ascribed to natural selection may have been formed by instinct developed by a faculty of evolution or expansion, establishing habits which have reacted on the organisation. In this way it is possible to account for the incipient stages of modifications which cannot at first have been of any use to their possessors, and the difficulties urged by Mr. St. George Mivart on this subject may be thus disposed of. I have not made the nature of the developmental agency an element or condition in any of the arguments I have hitherto used. I have reserved this subject for the concluding part of the paper.

The question arises whether this agency be molecular force alone or not? Because heat accompanies all organic operations, it is inferred, by those who hold the mechanical view of life, that heat is the sole agent in all processes. To refer embryonic changes to the action of heat simply because they take place only between certain limits of temperature is as illogical and as unwarrantable as it would be to consider the shape of a horse-shoe due to the same cause, for no better reason than that the application of a certain amount of heat from the furnace is necessary to its formation. A quotation in the first part of this paper suggested that the mechanical view of life would be demonstrated only when all the motions in an organism are shown to be the effects of forces which at other times are inherent in its atoms. It could not be expected that atoms should lose their physical properties on becoming component parts of an organised structure; moreover, molecules do not at all times possess the same power of arranging themselves as when they become the building materials of a developing embryo.

It is certain that heat and light play an important part in the economy of life; it is probable that they perform all the *work;* but there is not sufficient reason for believing that they alone effect the *evolution* of organisms. There are, however, many reasons for believing the contrary.

We know that no imaginable combination of chemical operations, no possible application of heat or light, can produce one drop of an organic acid from inorganic materials.

Prof. Tyndall,* M. Pasteur, and the Rev. W. Dallinger,

* In a recent lecture of Professor Tyndall's, the following remarks occur:—"From the beginning to the end of the inquiry there is not, as you have seen,

have furnished us with excellent reasons for believing that between living and not-living matter there is no link, but, on the contrary, an impassable barrier; that inanimate matter could, by no combination of chances, become animate; in short, that this world, destitute of life-germs, would, through all the changes that have taken place during the ages that have elapsed since it first became habitable, have remained an absolute desert, without a vestige of even the lowest forms of life. Thus, although it may never be actually demonstrated, evidence is already forthcoming of such a nature as to render it in the highest degree probable that spontaneous generation even from dead *organic* matter, is impossible at the present day; and if impossible now, it can never have taken place at any period of the world's history, *physical laws being unalterable*. The inconsistencies of Profs. Huxley and Tyndall, on this point, have been clearly indicated by Dr. Bastian, who remarks that:—"We find Prof. Tyndall also affirming in the most unhesitating language the ultimate similarity between crystalline and living matter: affirming that all the various structures by which the two kinds of matter may be represented are equally the 'results of the free play of the forces of the atoms and molecules' entering into their composition. And yet he, too, would have us believe that whilst differences in degree of molecular complexity alone separate living from not-living matter, the physical agencies which freely occasion the growth of living matter are now incapable of causing its origination." In another place he comments thus on Prof. Huxley's views:—"What reason does Prof. Huxley give, in explanation of his supposition as to the present non-occurrence of Archebiosis? He says, if it were given him 'to look beyond the abyss of geologically recorded time' to a still more remote period of the earth's history, he would expect 'to be a witness to the evolution of living protoplasm from not living matter.' And the only reason distinctly implied why a similar process should not occur at the present day, is because the physical and chemical con-

a shadow of evidence in favour of the doctrine of spontaneous generation. There is, on the contrary, overwhelming evidence against it; but do not carry away with you the notion, sometimes erroneously ascribed to me, that I deem spontaneous generation impossible, or that I wish to limit the power of matter in relation to life. My views on this subject ought to be well known. But possibility is one thing and proof is another; and when in our day I seek for experimental evidence of the transformation of non-living into the living, I am led inexorably to the conclusion that no such evidence exists, and that in the lowest, as in the highest, of organised creatures, the method of nature is that life shall be the issue of antecedent life."

ditions of the earth's surface were different in the past from what they are now. And yet, concerning the exact nature of these differences, or the degree in which the different sets of conditions would respectively favour the occurrence or arrest of an evolution of living matter, Prof. Huxley cannot possess even the vaguest knowledge. He chooses to assume that the unknown condition existing in the past were more favourable to Archebiosis than those now in operation. This, however, is a mere assumption which may be entirely opposed to facts. It is useless, of course, to argue upon such a subject, but still it might fairly be said, in opposition to his view of the impotency of present telluric conditions, that the abundance of *dead organic matter* now existing in a state of solution would seem to afford a much more easy starting-point for life-evolution than could have existed in that remote past, when no living matter had previously been formed, and consequently when no dead organic matter thence derived could have been diffused over the earth's surface. (This is a consideration of great importance; since those who believe that Archebiosis occurs in *organic* solutions at the present day have not yet professed to show that it can occur in *saline* solutions free from traces of organic matter.)

"Professor Huxley is, however, very inconsistent, since, in spite of his declared expectation of witnessing the evolution of living from lifeless matter, if it were given him to look beyond the abyss of geologically recorded time, he had said scarcely five minutes before, in reference to experimental evidence bearing upon the present occurrence of a similar process, that 'if, in the present state of Science, the alternative is offered us, either germs can stand a greater heat than has been supposed, or the molecules of dead matter, for no valid or intelligible reason that is assigned, are able to re-arrange themselves in living bodies; exactly such as can be demonstrated to be frequently produced in another way, I cannot understand how choice can be, even for a moment, doubtful.'"

The inconsistency pointed out by Dr. Bastian is undoubtedly very apparent. Whether Professors Tyndall and Huxley accept the law of Archebiosis for the past and reject it for the present, or, *vice versa*, they are equally inconsistent. Spontaneous generation is either a law of nature, or it is not. Imaginary telluric conditions in the ages that preceded geologically recorded time could not affect the question, *which must depend on the immutable properties of matter.* To those who do not believe in the present occurrence of

Archebiosis, there is but one legitimate course open—the rejection of the theory *in toto*. The subject being still *sub judice*, we do not insist on the impossibility of spontaneous generation. Even were it shown that the spontaneous generation of living things did occur at the present day from *inorganic matter*, it would merely lead us to believe that the organic developmental agency lies latent until called into activity by special conditions, *but not that it is identical with crystalline force*.

Professor Huxley remarks "that the property of crystallising is to crystallisable matter what the vital property is to albuminoid matter." But this analogy is not supported by facts. Crystallisable matter can never have crystallised in any other forms than those it assumes at present, and it must continue to crystallise in one set of forms only for all time; it can never develop into new shapes, but shall remain changeless for ever. Protoplasm, on the contrary, has, from the first moment it became active, never ceased to develop into countless varieties of organic structures, and is still developing into ever new and higher forms. While the distinguishing attribute of crystallisable matter is unchangeableness, that of albuminoid matter is continual progress. The action of the molecular forces on inorganic matter, under given conditions, can be predicted with the precision of abstract mathematics; but who shall foretell the forms of future living beings?

It seems highly probable, that in all organic matter there exists a controlling principle, directing the action of the molecular forces.

The organic machine is set in motion by heat, but its movements are regulated by a rigid law; it is constructed to perform a prescribed series of operations only. *The development of the germ may thus be regarded as due to self-acting and self-adapting mechanism of inconceivable refinement.* We find no force at work but molecular force, yet we observe phenomena that cannot be referred to the known properties of inorganic matter. We are therefore compelled (as the only apparent alternative) to believe in the automatic action of the forces we recognise; or in other words, that the development of organisms is due to a necessary sequence of operations, which must have for basis or starting point a certain definite and regular disposition of cells or molecules in the primordial germ; or, given protoplasm in a certain condition, development is effected by the laws of inorganic matter. This view of Evolution appears to negative the theory of Spontaneous Generation.

The fact already alluded to, that the development of an organism may be arrested either wholly or partially, and its growth continue as if no such arrest had taken place, has an important bearing on this subject, for it appears to show that development is distinct from the nutritive and reparative—in short, the purely mechanical processes of life—and that it is therefore due to an additional cause, the action of which may be suspended without arresting the ordinary physical processes.

The correlative development of all parts of an organism already referred to evinces systematic evolution. The convergence of all types to one, and the existence at the present day of forms closely allied to others long extinct, appears to indicate that organisms admit of development or evolution by certain invariable metamorphoses only.

We have no longer, at the present day, to concern ourselves with establishing the Evolution Hypothesis. Almost all those who are in a position to form a judgment are agreed in accepting it.

We must now confine ourselves to making a choice between two theories in explanation of the doctrine of descent. *First*, we may follow the Darwinians and believe that natural and sexual selection, habits, and other accidental circumstances are sufficient to account for all organic phenomena. If we maintain this, we are bound also to admit the purely mechanical view of life; because, in asserting the all-sufficiency of selection, &c., we preclude the action of all other agents with the exception of molecular force.

Again, if we adopt the mechanical view, we must, to be consistent, consider natural selection, &c., adequate to account for all forms of structure and all organic changes, because we eliminate all other possible agents—crystalline force, from its very nature, being wholly blind, casual, accidental, and in every way purely fortuitous. Furthermore, those who hold mechanical causes, directed by natural selection, &c., sufficient to account for the evolution of all animals and plants, are logically compelled to admit spontaneous generation as a necessary consequence; for otherwise there is imposed an arbitrary limitation to the extent or range of power of the molecular forces, and it is admitted that a first step is necessary, after which mechanical causes come into play—this is tantamount to acknowledging the existence of an agent separate and distinct from mechanical action. Thus the theory of selection, as sole agent of evolution, the mechanical view of life, and the theory of spontaneous generation, are inseparable; *they must stand or fall*

together, if one be proved invalid, all three are untenable. This position cannot be too strongly insisted on. Fully realised it would prevent much fruitless discussion. So-called Darwinianism is thus assailable on three points of great importance. Absolute proof or disproof of any of these is perhaps impossible, *but probability may be increased indefinitely in one direction or the other, so as virtually to amount to a certainty.*

Secondly, we may believe in a necessary and predestined sequence of molecular operations constituting a developmental law—assign to selection its legitimate work, modification, and adaptation, and reserve any expression of opinion as to the origin of life.

The question thus narrows itself to this: have organisms been developed by the selection of purely fortuitous aggregations of atoms with no other bond of union than reciprocal attraction and repulsion; or have they been systematically evolved by a pre-existent law? There seems to be but one difficulty in the latter suggestion—the origin of life; and this difficulty presents the only reason for accepting the first. Still it is possible that the power of organic development may have lain latent in inorganic matter since the universe itself was a germ—a primal haze—until such time as existing conditions favoured the beginning of life; but it seems more probable that protoplasm is coeval with inorganic matter, and that it primarily existed in what, for want of a better term, we may call a dormant condition. It has been fancifully and somewhat practically suggested* that the germs of life in our world may have sprung from some moss-grown fragment hurled from the ruins of another planet. This would, of course, be merely transferring the difficulty to some far-off world in which the origin of life must be as profound a mystery as it is in ours. In striving to find the truth we must not accept apparent possibilities, *or take refuge in illusory speculations as to the past condition of the earth.* We must divest ourselves of all preconceived ideas whatsoever, religious or scientific. We must not occupy ourselves too much with details, or special instances; but we must check our conclusions by the fundamental laws of nature.

The unfolding through unmeasured ages of the lowliest primordial forms, and their expansion to ever new and higher structures, resulting in the vast ascending series of plants and animals—the one with its interminable succession

* By Sir William Thomson.

of graceful forms and brilliant hues (forms and hues which it would be idle, even ridiculous to ascribe to utilitarian causes); the other, with its endless varieties of habits and instincts, its innumerable phases of organs, limbs, and systems, in which type is ever persistent and dominant, with its gradually increasing complexity of mechanism (in the contemplation of which the mind is unable to divest itself of the idea of design); all this, and more—the human intellect, human affections and aspirations—we must believe to be the results of *only a mode of motion*, or else we must acknowledge the presence of an influence the nature of which we can at presence but faintly attempt to conceive.

If we accept this latter view of evolution, the speculations of Mr. Wallace, and the views of Mr. Darwin, which Oscar Schmidt appears to consider regrettable, are seen to be the incomplete expression of, perhaps, the noblest conception of the nineteenth century.

NOTICES OF BOOKS.

The Effects of Cross- and Self-Fertilisation in the Vegetable Kingdom. By CHARLES DARWIN, F.R.S. London: John Murray.

THAT this work is intended as a contribution to the body of cumulative evidence already collected by Mr. Darwin in support of his views on the origin of species need not be questioned. But consisting as it does not of generalisation or controversial matter, but of the records of careful experiments and observations, it has a value quite independent of theories. Should hereafter the hypothesis of "natural selection" be superseded, should the doctrine of Evolution in any shape be abandoned, the volume before us must still remain a highly important contribution to biological science.

The author on the threshold of his enquiry points to the abundant evidence that flowers are constructed so as to be cross-fertilised, occasionally or habitually, by pollen from another flower, whether growing on the same or on a different plant. To ensure such cross-fertilisation a number of curious arrangements exist, which the author and other observers have elsewhere described, and to which he only therefore refers in passing. We will merely remind the reader that this purpose is in some cases secured by a separation of the sexes of flowers, whilst in others the pollen and the stigma of the same flower are not matured at the same time. Sometimes the impregnation of flowers by their own pollen is prevented, or at least impeded, by beautiful mechanical contrivances. In one class the ovules "absolutely refuse to be fertilised by pollen from the same plant, but can be fertilised by pollen from any other individual of the same species."

Mr. Darwin's present concern is not with the means, but with the ends of cross-fertilisation. It would be "simpler," surely, for every plant to have been fecundated by its own pollen; but finding this state of things in a number of cases so carefully guarded against, we are warranted alike on the principles of the Old and the New School of Natural History in supposing that we have before us no mere accident. The author was led to undertake the experiments hereinafter detailed by the following circumstance:—For the sake of determining certain points with respect to inheritance, and without any thought of the effects of close inter-breeding, I raised close together two large beds of self-fertilised and crossed seedlings from the same plant of *Linaria vulgaris*. To my surprise the crossed plants, when

fully grown, were plainly taller and more vigorous than the self-fertilised ones." Mr. Darwin, however, whom some persons accuse of rushing hastily to conclusions without sufficient evidence, considered it still "quite incredible that the difference between the two beds of seedlings could have been due to a single act of self-fertilisation." The next year he performed an analogous experiment. "I raised, for the same purpose as before, two large beds close together of self-fertilised and crossed seedlings from the carnation, *Dianthus caryophyllus*. This plant, like the *Linaria*, is almost sterile if insects are excluded, and we may draw the same inference as before, namely, that the parent-plants must have been inter-crossed during every—or almost every—previous generation. Nevertheless, the self-fertilised seedlings were plainly inferior in height and vigour to the crossed."

A formal series of experiments was then undertaken with various plants, and was continued for eleven years, the crossed plants in the great majority of cases being found to have the advantage. The general mode of experimentation was as follows:—"A single plant, if it produced a sufficiency of flowers, or two or three plants were placed under a net stretched on a frame, and large enough to cover the plant without touching it. This latter point is important, for if the flowers touch the net they may be cross-fertilised by bees. I used at first white-cotton net with very fine meshes, but afterwards a kind of net with meshes one-tenth of an inch in diameter. On the plants thus protected several flowers were marked, and were fertilised with their own pollen; and an equal number on the same plant, marked in a different manner, were at the same time crossed with pollen from a distinct plant. The crossed flowers were never castrated, in order to make the experiments as like as possible to what occurs in Nature with plants fertilised by the aid of insects. In some few cases of spontaneously self-fertile species the flowers were allowed to fertilise themseves under the net, and in still fewer cases uncovered plants were allowed to be freely crossed by the insects which incessantly visited them."

The seeds from the flowers thus treated were allowed to ripen thoroughly, and were then allowed to germinate, with the following precautions:—"The crossed and self-fertilised seeds were placed on damp sand, on opposite sides of a glass tumbler covered by a glass plate, with a partition between the two lots, and the glass was placed on the chimney-piece in a warm room. I could thus observe the germination of the seeds. Sometimes a few would germinate on one side before any on the other, and such were thrown away. But as often as a pair germinated at the same time they were planted on opposite sides of a pot, with a superficial partition between the two; and I then proceeded until from half-a-dozen to a score or more seedlings, of exactly the same age, were planted on the opposite sides of several pots.

If one of the young seedlings became sickly or was in any way injured it was pulled up and thrown away, as well as its antagonist on the opposite side of the same pot.

"As a large number of seeds were placed on the sand to germinate, many remained after the pairs had been selected; these were soon crowded together on the opposite sides of one or two rather large pots, or sometimes in two long rows out of doors. In these cases there was the most severe struggle for life among the crossed seedlings on one side of the pot and the self-fertilised seedlings on the other side, and between the two lots which grew in competition in the same pot. A vast number soon perished, and the tallest of the survivors on both sides when fully grown were measured. Plants treated in this manner were subjected to nearly the same conditions as those growing in a state of Nature which have to struggle to maturity in the midst of a host of competitors."

Sometimes the seeds, instead of being previously allowed to germinate on damp sand, were sown at once on opposite sides of pots, and the plants measured when fully grown. This plan Mr. Darwin pronounces less accurate, as the seeds sometimes germinated more quickly on one side than the other. He considers, however, that it was necessary thus to proceed in the case of some few species, "as certain kinds of seeds would not germinate well when exposed to the light." We should suggest that such seeds might have been covered with a plate of blue glass, since blue light—though not favourable to plants in the later stages of their life—undoubtedly promotes germination.

Every precaution was taken that the two classes of seedlings under comparison should in all other respects be precisely on an equality. The soil was evenly and thoroughly mixed, the supply of water and the exposure to light were the same. Yet, as we have already intimated, the self-fertilised plants, when carefully weighed and measured, were decidedly inferior to the crossed.

Hence Mr. Darwin is perfectly justified in the inference that cross-fertilisation is generally beneficial and self-fertilisation injurious. "That certain plants," he remarks, "such as *Cyclamen persicum*, &c., which have been naturally cross-fertilised for many or all previous generations, should suffer to an extreme degree from a single act of self-fertilisation is a most surprising fact. Nothing of the kind has been observed in our domestic animals; but then we must remember that the closest possible interbreeding between such animals—that is, between brothers and sisters—cannot be considered as nearly so close a union as that between the pollen and the ovules of the same flower. Whether the evil from self-fertilisation goes on increasing during successive generations is not as yet known, but we may infer from my experience that the increase, if any, is far from rapid. After plants have been propagated by self-fertilisation for several generations, a single cross with a fresh stock restores their pristine

vigour; and we have a strictly analogous result with our domestic animals."

The question whether a vegetable species can be reproduced asexually, *i.e.*, by rhizomes, stolons, &c., from a very remote period remains open. Andrew Knight maintained that a variety exclusively thus propagated, like the majority of our fruit trees, must ultimately become weakly, and Prof. Asa Gray leans to the same view. It would be interesting, if the Anti-Vivisectionists would allow it, to take some animal capable of propagation by "cuttings," and try for how many generations this mode of reproduction could be carried on without a visible decay of vigour.

Mr. Darwin guards against the inference that cross-fertilisation is, *per se*, beneficial under all circumstances. His experiments show that the "benefit from cross-fertilisation depends on the plants which are crossed having been subjected during previous generations to somewhat different conditions." Thus plants which had been self-fertilised for the eight previous generations were crossed with plants which had been inter-crossed for the same number of generations, all having been kept under the same conditions as far as possible; seedlings from this cross were grown in competition with others derived from the same self-fertilised mother-plant crossed by a fresh stock, and the latter seedlings were to the former in height as 100 : 52, and in fertility as 100 : 4." The advantages of a cross, Mr. Darwin considers, "depend altogether on the differentiation of the sexual elements, a conclusion which harmonises perfectly with the fact that a slight and occasional change in the conditions of life is beneficial to all plants and animals. We thus see that in many species plants fertilised with their own pollen are either absolutely sterile or very sparingly fruitful; if fecundated with pollen from another flower on the same plant, they are sometimes a little more fertile; if treated with pollen from another individual or variety of the same species, their fertility is at its maximum; but if with pollen from a different species their fertility declines, till we arrive at absolute sterility. "We have thus a long series with utter sterility at the two ends; at one end due to the sexual elements not having been sufficiently differentiated, and at the other end to their having been differentiated in too great a degree or in some peculiar manner." But having penetrated so far we must confess our ignorance. "We do not know what is the nature or degree of the differentiation in the sexual elements which is favourable for union, and what is injurious." Some species are greatly benefitted by crossing, while others profit very little. Some plants retain their vigour after having been self-fertilised for untold generations. But for these and for many connected facts we can scarcely conjecture a reason.

Going still further, and admitting—as we are compelled—that fertile eggs can be produced without the co-operation of the male,

we ask, why have the two sexes been developed? Mr. Darwin finds the answer in the fact that the offspring of two distinct parents, especially if descended from stocks exposed to somewhat dissimilar conditions, have an advantage in vigour over the progeny of a single self-fertilised individual. But if, as appears probable, the sexes were primordially separate, why did they become blended into hermaphrodite forms, and why—in all the higher animals and in some plants—have the sexes again been separated? The bilateral structure of animals, as Mr. Darwin suggests, perhaps indicates that they were aboriginally formed by the fusion of two individuals. In connection with this subject we have had occasion to refer to certain curious cases of bilateral hermaphroditism found among moths, where one wing, antenna, &c., bear the characters of the male, whilst the other side is as plainly female. But we have vainly sought for any analogous instance either among other insects or among birds and mammals.

The whole tendency of these researches, when calmly and impartially weighed, must be to shake the confidence commonly felt in the primordially distinct character of "species" as compared with mere varieties. The difference in the affinities of the sexual elements of different species, on which their mutual incapacity for breeding together depends, is caused by their having been habituated for a very long period each to its own conditions and to the sexual elements having thus acquired firmly fixed affinities."

The Various Contrivances by which Orchids are Fertilised by Insects. By CHARLES DARWIN, F.R.S. Second Edition, Revised. London: John Murray.

THIS work is already too widely and too favourably known to require examination or comment. The present edition has been, as the author informs us, enriched with many new and curious facts communicated by correspondents in different parts of the world, among whom especial mention is made of Dr. Fritz Müller. A few errors have also been corrected. We cannot help regarding it as a somewhat unfortunate omission that the author has not given a list of the additions and modifications introduced into the present edition. It must, however, be distinctly understood that the alterations thus made are far from invalidating the conclusions reached in the former edition. A list is appended of all the memoirs and books bearing on the fertilisation of the Orchideæ which have appeared since the first appearance of the present work, in 1862. It is somewhat singular that, whilst the botanists of England, America, Germany, and Italy have laboured diligently in the investigation of this

interesting question, those of France still hold aloof, and contribute nothing towards the solution of the problems here stated or suggested. For instance, it may well be asked why, in spite of all the wonderful contrivances for fertilisation which we are compelled to recognise, so few of the seeds of the Orchids are really productive? According to Mr. Scott a single plant of an *Acropera* may sometimes yield seventy-four millions of seeds. In a single capsule of a *Maxillaria* Fritz Müller found 1,756,440 seeds. Yet some unknown cause checks their multiplication, so that, despite the astonishing number of their seeds, they are as a rule sparingly distributed. In no country is the number of individuals of any one species nearly so great as that of very many other and far less prolific plants.

The following fact deserves to be seriously considered by all who are engaged in experimenting on the part played by insects in the fertilisation of plants. According, namely, to Mr. Moggridge, "*Ophrys scolopax* fertilises itself freely in one district of Southern France without the aid of insects, and is completely sterile without such aid in another district."

The seventh chapter of the book, treating of the fertilisation of the *Catasetidæ*, is strangely suggestive. The flowers of the male plant, if touched at certain definite points by an insect, shoot forth their pollinia, which, being furnished with excessively adhesive points, cling to the intruder, and are by him carried to a female plant. If this is mere automatism, where in the organic world are we to draw a sharp boundary line between such mere mechanical action and the "instinctive" performances of the lower animals, or even of man? But if there be nothing automatic in the latter, can we venture to deny that the plant may also have its instincts, and even its dim self-consciousness? What if the old myth of the hamadryads foreshadowed a great truth?

Through Norway with Ladies. By W. Mattieu Williams, F.R.A.S., F.C.S., &c. London: E. Stanford.

The mere title of this book will at once reveal its origin and secure for it a favourable reception. Nor will such persons as are led to take up the work, by their pleasant recollections of its companion volume, find themselves disappointed. Mr. Williams journeys not to have "done" certain localities, but to see and to learn: he finds interesting facts which too many ramblers overlook; he draws valuable lessons from phenomena apparently threadbare, and he lays before us his observations and reflections in a genial and pleasing manner, and without "posing" for our admiration. If the work before us does not equal certain records of travel which have appeared within the last quarter of a

century, the cause must be sought out not so much in any shortcoming of the author as in the nature of his subject. The *fauna* and *flora* of Norway are comparatively poor, and differ little from those of the rest of Northern Europe, and its natural phenomena in general have been well and often investigated. That Mr. Williams has been able to secure such valuable gleanings from so ably reaped a field makes us regret the more that he has not turned his attention in preference to some richer and less-known region. But his tastes are evidently ultra-Hyperborean; he advocates "Arctic Expeditions for the Million," and declares that he should hugely enjoy an excursion up to 81° 30′ N. lat., the bare idea of which causes our flesh to creep.

Among the abundance of interesting matter it is not easy to make a selection. So plunging at once *in medias res* we are reminded that Norway is the classical home of the sea-serpent. No scientific traveller can visit the fjords without attempting to solve this mystery and to account for its alleged existence. Our author's conjecture, though we cannot accept it, must be pronounced bold and original. Observing the existence of certain low interrupted ridges of rock stretching out into the sea, he thinks that these, under certain atmospheric conditions, may have been mistaken for the coils of an enormous serpent, "floating many a rood." It must, of course, be admitted that the currents of air which in bright sunny weather may be perceived rising up from a heated surface may give a semblance of motion to a fixed and inanimate object. It will also be granted that, according to Pontoppidan and other the like authorities, the sea-serpent was in the habit of appearing only in calm, sunny weather, in the height of summer, and of sinking down out of sight as soon as a slight wind happened to spring up. So far the features of the case are in favour of our author. But his supposition would, at best, only account for the appearance of the supposed monster in some very few localities. Further, we cannot go so far as to assume that fishermen—familiar, from their youth up, with the existence of the rocks in question and with their aspect in every kind of weather—could be thus easily deceived. Suppose the sun to become overcast, or a wind to spring up, the undulations of the air would of course cease, and the seemingly life-like movements would be at an end. But the rocks would remain in their wonted places, and the spectators—instead of concluding that a serpent previously present, in addition to these rocks, had just disappeared—would far more probably draw the correct inference, that they had mistaken a motionless ridge for a living creature.

Let it not be for a moment supposed that we hold a brief for the sea-serpent. The existence of a marine Ophidian surpassing the python and the boa in size, in the same proportion as the whale exceeds the elephant, has become too improbable to be

established by any evidence short of its actual capture and exhibition. Were we to have an opportunity of observing the supposed monster, even at a very short distance, we should keep complete silence on the interview. But Mr. Williams's explanation seems to us as little satisfactory as the hypotheses of floating barrels, timbers of a wrecked vessel, with or without sea-weed attached, gigantic seals, sharks, &c., which have been put forward by other writers. It is simpler and safer to explain the alleged phenomena by the aid of "unconscious cerebration." The observers, whose "early scientific education" had probably in all cases been neglected, have allowed themselves to become the slaves of a "DOMINANT IDEA."

On the colour of water we find some interesting remarks. The author considers that "after deducting the influence of the reflected light of the sky or the clouds, the residual special colour is *directly* due to the minute particles suspended in the water." As instances of the colourless nature of very pure, even though deep, waters, he mentions the Aachensee in the Tyrol, and the Anapo near Syracuse. "Floating in a boat over the fountain of Cyane, the source of the Anapo, and looking down at the pebbles, seen with microscopic distinctness 40 or 50 feet below, was suggestive of sitting in the car of a balloon." In comparison with these waters he considers the "green Rhine and the blue Rhone" both lacking in transparency, their different shades of colour being due to suspended rock particles. The deepest purple or indigo waters he has found in the neighbourhood of dark slaty rocks. In limestone districts, and amidst red and deep yellow sandstones, the waters are green. The waves of the Atlantic along the west coast of Ireland are "more deeply indigo in tint than out at sea," owing to a vast quantity of suspended dark purple rock particles, too small to be separately visible, even with the aid of the microscope. Waters flowing through peat bogs are brownish when shallow, but pitchy black when deep, and especially when seen in the shade. This colour the author believes is actually due to a kind of pitch derived from the peat.

With reference to the profusion of cherries, currants, and gooseberries in Norway, Mr. Williams seeks the cause in the absence of sparrows and other strong-billed birds. Hence he dissents from those writers who denounce sparrow-clubs, and he defends the English farmer against the charge of being an ignorant and indiscriminating murderer of all small birds. To a great extent he is here in the right: the exportation of sparrows to Australia was a folly and a crime only one degree smaller than the introduction of rabbits, goats, and swine into uninhabited islands, or the proposal to supply a "constitutional check" to the first of these three destructive animals by the importation of polecats. The chief difficulty of the case is the impracticability of at once extirpating the sparrows and preserving the insect-

eating birds. The decrease of many insectivorous species is due not so much to any act of the farmers as to nest-hunting boys, holiday sportsmen, and to those professional bird-catchers who are found in all our large towns, and who, in utter defiance of the statute decreeing a "close time" for small birds, extend their ravages for fifty miles around London.

If a digression may be here permitted, we are informed that the starling—a purely insectivorous and most useful bird, and one which Mr. Williams will find eats larvæ not merely when hard pressed for food—is debarred from even the doubtful protection of the Wild Birds Protection Act. The reason assigned is, that it is sometimes selected as the victim of those eminently British institutions, gun-clubs. In this humanitarian, anti-vivisectionist, and moreover thoroughly practical country, the infliction of pain and death—even in defiance of utilitarian considerations—is a sacred institution if wanton amusement is the object; but if valuable knowledge is sought for the deed becomes an "orgy of diabolism."

Like many other travellers, from the time of Linnæus downwards, Mr. Williams was annoyed by the musquitoes of Lapland, which are here no less numerous than in any tropical swamp. To explain their abundance in these high latitudes he points to the total absence of swallows. That these birds do contribute powerfully towards the reduction of such minute vermin is undoubted; but musquitoes, unfortunately, are quite able to co-exist with swallows; indeed, varying in name and in trifling morphological details much more than in malignity, they infest upwards of three-fourths of the land on this our globe, and afford to the teleologist and the optimist a riddle which has not yet found its Œdipus.

Our author's main object, in both his visits to Norway, appears to have been the study of the so-called Glacial epoch. On this subject he has reached conclusions different from those formed by certain other geologists. He does not at all question the former "existence of two or more such epochs during the later period of the Tertiary age, with an intervening warm period or periods." But he considers that the magnitude of the phenomena has been greatly exaggerated. He quotes from Mr. Geikie's "Great Ice Age" (p. 561) a passage which we may here reproduce for the convenience of the reader:—"All Northern Europe and Northern America disappeared under a thick crust of ice and snow, and the glaciers of such regions as Switzerland assumed gigantic proportions. This great sheet of land ice levelled up the valleys of Britain, and stretched across our mountains and hills down to low latitudes in England. Being only one connected or confluent series of mighty glaciers, the ice crept ever downwards and outwards from the mountains, following the direction of the principal valleys, and pushing far out to sea, where it terminated at last in deep water, many miles

away from what now forms the coast-line of our country. *This sea of ice was of such extent that the glaciers of Scandinavia coalesced with those of Scotland and the north-eastern districts of England, upon what is now the floor of the shallow North Sea*, while a mighty stream of ice, flowing outwards from the western sea-board, obliterated the Hebrides, and sent its icebergs adrift in the deep waters of the Atlantic."

In Mr. Williams's opinion the configuration of the Lofodens decidedly contradicts the passage put in italics. The glaciers swept over the inner portion of this group, as is proved by the rounded, "hog's-back" form of the small rocky islets between Saltenfjord and Röst, but thinned out to seaward and failed to reach the rocks of Röst, which show no marks of the grinding-down action of ice-masses. Hence the author infers that the glaciers were insufficient to push fifty miles out to sea, "unless we suppose the submergence of the land to have been so great that the out-thrust *mer de glace* floated over these low rocks without grazing them." He concludes that "*even at the bitterest period of greatest European glaciation the waters of the ocean within the Arctic circle were warm enough to thaw, with considerable rapidity, the ice which had accumulated on the mountains and in the valleys of the land.*"

The aspect of the North Cape and of the other headland along the northern face of Europe seems also, to the author, to refute the idea of a great ice-sheet having "proceeded radially from the north polar regions, and overswept Scandinavia and the rest of Northern Europe." He finds that the "craggy headlands and the structure all indicate that the ice-sheet moved in the contrary direction, from south to north." It may, he admits, be urged in opposition that weathering has produced the precipitous character of the headlands since the Glacial epoch. This, however, he argues is contradicted by the "absence of a sea-beach such as would be formed by the material washed down by the waves had they done the great amount of work necessary for the conversion of glaciated slopes into precipices rising above 1000 feet out of the water." The till he considers to have been deposited from the under surface of glaciers not resting upon the ground, but floating in shallow water. The difficulty of the ground moraine theory, he thinks, is that of reconciling the existence of such a deposit as the till "with the tremendous erosive power of the glacier moving over it; especially when we consider that at the time of its deposit from the thawing ice, the matrix, the present stiff clay must have been a *purée* of thin slimy mud."

In the ice-age he holds that the seas adjacent must have been much deeper than at present. Not only would the earth's centre of gravity be, he contends, affected by the piled-up masses of ice and snow, but these very masses must, by their own gravitation, raise the nearest sea-level. He quotes Prof. Ramsay to the effect that the "probable submergence during some part of the

Glacial epoch may be taken at about 2300 feet." Mr. Belt, on the other hand, holding that the whole globe was glaciated not by alternate hemispheres, but simultaneously, considers that the amount of water abstracted from the ocean, and piled up in a solid form upon the continents, must have been so considerable as to reduce the general sea-level and lay bare lands now submerged to depths ranging from 1000 to 2000 feet, which would afford a temporary asylum for the tropical flora and fauna.

Mr. Williams seeks the cause of glacial epochs simply in alternating lengths of summer and winter in the two hemispheres. He holds that "no shifting of the earth's axis, no extreme convulsion, is demanded; for if, under the present distribution of terrestrial climate, the southern hemisphere had as much land around its poles as the northern hemisphere now has, it would be glaciated from the South Pole down to the latitude corresponding to that of London." It appears to him that "if we simply add the changes of climate which, as Lyell has shown, must of necessity result from variations in the distribution of land and water to the similar changes which must follow as necessary results of the known and demonstrable variations of the earth's orbit, we shall have in this combination an agency of sufficient energy to explain all the known phenomena." In his former work, "Through Norway with a Knapsack," Mr. Williams has contended that "low temperature is only one of the factors in the formation of glaciers, and that an increase of atmospheric humidity, and of consequent winter snow-fall, may produce the same effect as lowering of temperature." He supposes, indeed, that in the ice-age "the temperature of the northern hemisphere was lower than at present, but not nearly so low as is commonly supposed; and that this moderate depression of temperature, combined with a greater snowfall in winter, produced all the observed results. The lower temperature would favour increased precipitation, and the increased humidity of the air would resist the passage of the solar rays and greatly diminish the summer thawing."

According to Mr. Williams, then, the southern hemisphere is now "enjoying" its glacial epoch, though not in a very aggravated form. But it is possible, or rather highly probable, that an agency which he leaves out of account may very much modify the character of these ice-ages. The orbit of the earth varies periodically, alternately approaching nearer to a circle, and then becoming more eccentrically elliptical. This cause, as Mr. Croll has shown, will sometimes greatly intensify the influence of the shortened summer, and may thus produce epochs of a more intensified glaciation.

There are other subjects, not a few, on which our author expresses himself in an original and interesting manner, but they are not adapted for discussion in the "Quarterly Journal of Science." We will therefore conclude by thanking him for the

pleasure and instruction we have derived from the perusal of his book, expressing our conviction that most of his readers will have occasion to pay him a similar tribute.

The Amateur Mechanics' Practical Handbook. By ARTHUR H. G. HOBSON. London: Longmans and Co.

THIS little manual treats, in successive chapters, on the lathe and its uses; on the drilling and planing machine; on the vice, bench, and hand-tools; on drawing and pattern-making; on the brass furnace and moulding; on the construction of horizontal engines; and on boilers. The author declares that his object has been to give plain, practical instructions, avoiding intricate subjects. Previous works which have dealt more or less prominently with this subject give, he finds, very vague instructions, or else are too theoretical to be of much use to the amateur—charges which we think are not unfounded. The directions contained in this little volume are full and precise, and are illustrated with numerous figures. We believe that Mr. Hobson has, by the publication of this treatise, conferred a boon upon amateur mechanics—a somewhat numerous class.

The Elements of Machine Design: an Introduction to the Principles which Determine the Arrangement and Proportions of the Parts of Machines, and a Collection of Rules for Machine Design. By W. CAWTHORNE UNWIN, Professor of Hydraulic and Mechanical Engineering at the Royal Indian Civil Engineering College. London: Longmans and Co.

THIS work belongs to the series of "Text-Books of Science" issued by Messrs. Longmans, though by the strict methodologist it would be pronounced as belonging to the domain not of *science*, but of *art*—a distinction too commonly lost sight of, and, if we may judge from the last sentence of the Preface, not fully comprehended by the author.

In the first chapter Prof. Unwin treats of the materials used in the construction of machines. Here the properties of the various kinds of cast- and of wrought-iron are carefully explained. Phosphor-bronze is noticed at some length, the author expressing the opinion that it is likely to be of great service in machine construction. Its chief drawback in the eyes of the chemist is that its manufacture withdraws a portion of phosphorus from natural circulation. Might not an alloy of similar properties be obtained by substituting arsenic for phosphorus?

In successive chapters the author considers the straining action to which machines are subject; the strength of materials, fastenings, pipes, and cylinders; of pivots, axles, and shafting; of bearings for rotating-pieces; of toothed, belt, and rope gearing; of link-work; and of pistons, valves, and cocks.

The work may be fairly pronounced to have no small share of the invaluable attribute of thoroughness, and we think that it is very well calculated to effect its primary object. This, as the author tells us, is "to explain the principles that are available as guides in machine-construction. So far as it succeeds in this it will place the draughtsman in the best position to make use of the facts which come under his notice in the workshops." We must call particular attention to these last few words, because they seem to us to summarise the whole purpose of technical education. Every artizan ought to be so trained that he may make use of such facts. To this end he must be taught to observe, in order that the facts may not altogether escape him, and he must be acquainted with the scientific principles on which his art is based, in order that he may know how to deal with such facts.

The Ancient Life-History of the Earth, a Comprehensive Outline of the Principles and Leading Facts of Palæontological Science. By H. Alleyne Nicholson, M.D., D.Sc. Edinburgh and London: Blackwood and Sons.

The especial object of the work before us, as we are told in the Preface, is the study of fossil animals from the historical point of view, regarding them "principally as so many landmarks in the ancient records of the world," and examining "their relations to the chronological succession of the strata in which they are entombed." Their morphology and their relation to existing forms of life occupy here a mere subordinate place.

This preliminary demarcation of his subject having been drawn, the author enters upon the consideration of the laws of geological action. Happily observing that, in its strict sense, "Geology is nothing more than the Physical Geography of the past, just as Physical Geography is the Geology of to-day," he discusses, in a most philosophical spirit, the two grand conflicting theories of Catastrophism and Uniformitarianism. Here, whilst insisting that the present state of our earth is "really the result of the tranquil and regulated action of known forces through unnumbered and innumerable centuries,"—that "the history of the earth has been one of *law* in all past time, as it is now,"—that the successive groups of animals and plants revealed to us in turning over the pages of the "stone book" are, "to a greater or less extent, directly connected with one another, each being

the lineal descendant of the group which immediately preceded it in point of time," and being "more or less fully concerned with giving origin to the group which immediately follows it,"—he yet reminds us that "from one point of view there is a truth in catastrophism which is sometimes overlooked by the advocates of continuity and uniformity. Catastrophism has, as its essential feature, the proposition that the known and existing forces of the earth at one time acted with much greater intensity and violence than they do at present, and they carry down the period of this excessive action to the commencement of the present terrestrial order. The Uniformitarians, in effect, deny this proposition, at any rate as regards any period of the earth's history of which we have actual cognisance. If, however, the 'nebular hypothesis' of the origin of the universe be well-founded, as is generally admitted, then, beyond question, the earth is a gradually cooling body, which has at one time been very much hotter than it is at present. There has been a time, therefore, in which the igneous forces of the earth, to which we owe the phenomena of earthquakes and volcanoes, must have been far more intensely active than we can conceive from anything that we can see at the present day. By the same hypothesis the sun is a cooling body, and must at one time have possessed a much higher temperature than it has at present. But increased heat of the sun would seriously alter the existing conditions affecting the evaporation and precipitation of moisture on our earth, and hence the aqueous forces may also have acted at one time more powerfully than they do now. The fundamental principle of catastrophism is therefore not wholly vicious; and we have reason to think that there must have been periods—very remote it is true, and perhaps unrecorded in the history of the earth—in which the known physical forces may have acted with an intensity much greater than direct observation would lead us to imagine."

The truth in these remarks is undeniable; but it strikes us that the difference between these two conflicting views resolves itself after all into the question, When was the "commencement of the present terrestrial order?" Catastrophists admit that the world is *now* governed by law, and that we find merely gradual change, and no evidence of arbitrary intervention, of alternating miracles of destruction and re-creation. But can they fix the date when the "*now*" began? On the other hand, Uniformitarians are quite willing to admit that in the pre-geological ages—which, as Dr. Nicholson intimates, have passed "unrecorded in the history of the earth"—all agencies whose effects we now witness must have been far more powerful than at the present day. But in admitting this we urge that the decline of such forces must have been very gradual, that the difference between *then* and *now* must have been in degree and not in kind, and that consequently, as the author shows, the history of the earth has

always been one of law. Yet Catastrophism overlooks this fundamental fact. It is curious to look back to the assumptions devised by Catastrophists and Brachychronologists to explain away the evidence of the earth's past. Thus Sir David Brewster, whilst fully admitting the traditional longevity of antediluvian man, suggested—to account for the formation of the coal-strata within the six thousand years of what was once the received chronology—that organic life was "once upon a time" briefer and more rapid than it is now, and that whole forests might have sprung up to maturity and passed away in fewer years than they would now require centuries." This introductory section of the present work may be most advantageously studied, even by those who may feel no special interest in the details to follow.

As regards the body of the work, space will not allow us to examine it minutely, chapter by chapter. The facts adduced are, of course, to a great extent the common property of geologists. But even the veriest tyro must, we should think, feel that he is under the guidance not of a compiler stringing together the most interesting—we had almost said the most sensational—matter upon which he has stumbled whilst "reading up" the subject, but of a master in the science who understands the significance of every phenomenon which he records, and knows how to make it reveal its lessons.

The cautions given concerning the possible sources of error in palæontological inquiry are well worthy the attention of the student. Dr. Nicholson reminds us of the frequent absence of fossils in stratified rocks; of the confusion caused by observers in different parts of the world giving different names to one and the same fossil, or, conversely, "lumping" distinct fossils under some common name. These errors are naturally dangerous whenever we argue as to the comparative age of strata from the presence of certain fossil remains. He further shows that the mere fact of a difference of physical position cannot safely be taken into account in determining the true zoological affinities of a fossil. Unsafe, also, is the inference that all beds containing similar fossils must be of the same age. The palæontologist may also be led astray by accepting obscure or imperfect physical evidence as to the age and position of some given group of strata.

Concerning the inferences drawn from fossil remains as to the former climate of the locality where they are found, some important cautions are given. The author points out that, as fossils are mostly the remains of marine animals, the evidence obtained bears chiefly upon the temperature of the seas, which, from the existence of currents, hot or cold, does not necessarily prove a corresponding temperature in the adjacent lands. It is, moreover, by no means certain that the "habits and requirements" of any extinct animal were exactly the same as those of its nearest living representatives. Nor is the distribution of species

at the present day dependent upon conditions of climate alone. He does not, however, at all question the conclusion, supported as it is by a vast accumulation of evidence, that the climate of Central Europe in the Eocene period was tropical, whilst that of the Miocene approximated in temperature to Florida or Louisiana.

The view, once prevalent, that fossils are not the remains or casts of animals once living, but *lusus naturæ* formed by some "plastic virtue latent in the earth," or, worse still, as Mr. Gosse most irreverently, in our opinion, insinuates, forged documents to mislead mankind, Dr. Nicholson pronounces contrary to the common sense of scientific men.

The chapter on the "Biological Relations of Fossils," and still more that on the "Succession of Life upon the Globe," bring the author face to face with the great biological question of the day, the origin of species. His views on this subject attract the more interest as he has been to some extent misunderstood, if not misrepresented. Certain of the remaining advocates of the old school of Natural History have invoked him as a kind of successor to Agassiz and an ally of that redoubted dispenser of teleology and magniloquence, Principal J. W. Dawson. But we see no ground on which they can claim Dr. Nicholson as belonging to their fraternity. He decidedly recognises organic evolution. "The palæontologist," he writes, "is so closely confronted with the phenomenon of closely-allied forms of animal life succeeding one another in point of time, that he is compelled to believe that such forms have been developed from some common ancestral type by some process of evolution." If he adds that there "has also been at work some other deeper and higher law on the nature of which it would be at present futile to speculate," he says little more than what every cautious thinker must admit. Natural selection, sexual selection, changing climatic influences, disuse of organs, explain much. Do they explain all? That Dr. Nicholson can rank among the opponents or enemies of Darwin—for there are those who deserve the latter rather than the former appellation—no one who reads the final sentence of this book can admit. "In the successful solution of this problem will lie the greatest triumph that palæontology can hope to attain; and there is reason to think that, thanks to the guiding clue afforded by the genius of the author of the 'Origin of Species,' we are at least on the road to a sure, though it may be a far distant, victory."

The illustrations are numerous, carefully executed, and judiciously selected. To each period is appended a list of the principal works and memoirs which the student may require to consult for more detailed information. There is also an elaborate glossary of technical terms employed, an extensive index, and a "tabular view of the chief divisions of the animal kingdom," in which, however, we regret to perceive that the Mollusca are

made to rank higher than the Annulosa, and that the order Bimana is still retained—two relics of Cuvierianism scarcely on a level with the general standard of the author's teachings. As regards the value of the work there can scarcely exist two opinions. As a text-book of the historical phase of palæontology it will be indispensable to students, whether specially pursuing geology or biology, and without it no man who aspires even to an outline knowledge of Natural Science can deem his library complete.

A New London Flora, or Handbook to the Botanical Localities of the Metropolitan Districts. Compiled from the Latest Authorities, and from Personal Observation, by EYRE CH. DE CRESPIGNY, M.D. London: Hardwicke and Bogue.

THE author of this little manual remarks very truly that a new London Flora is much required. The suburban localities for rare or interesting species, such as we knew them in our youth, exist merely in tradition. They have been desecrated by the land-speculator and the "navvy." Woodland, heath, and common now produce little save a crop, more pretentious than interesting, of semi-detached villas. Even where the plague of bricks and mortar has been averted, every wild flower of at all striking appearance, and well nigh every fern save the *Pteris aquilina*, have been long since dug out and carried off to be sold. As the author remarks, "It is a question now-a-days not what there is, but what there is not in the way of rarity." The London district, however, within the average thirty-mile radius taken in this manual, is still rich in species. Of the total 1665 plants which—exclusive of Charads and minor Cryptogams—occur in all Britain—no fewer than 1250 occur in the metropolitan region. Here, however, both author and reader are brought in contact with the vexed question, What is a species? Dr. de Crespigny evidently does not agree with the so-called "splitters," who would elevate every variety to the rank of a species. But for this evil—as it undoubtedly is—the remedy is still to seek. Given a certain group of closely-allied forms, animal or vegetable, one observer may refer the specimens laid before him to ten species, whilst another may conceive that he has proved the existence of twenty, and neither can convict his opponent of error. Does not all this help to undermine the current idea of "species" as an immutable and objective reality?

The author first gives an alphabetical list of species, with their time of flowering and with their localities, save, of course, such as are to be found everywhere. The other main section of the work is devoted to a description of the chief botanical localities, with an enumeration of their productions. We notice

here a mention of Coombe Wood, once so dear both to the botanist and the entomologist, but now shut against both. The author generally mentions such woods and heaths as are closed against scientific exploration—a very useful feature in the manual, which may save many a student from wasting a day. Such restrictions, as far as uncultivated lands are concerned, are, we believe, peculiar to the United Kingdom. Among minor features of the book may be mentioned a list of the authorities consulted, and a list of aggregates, segregates, and synonyms.

To all botanical students residing in the London district this manual will prove a useful guide.

Ferns, British and Foreign: the History, Organography, Classification, and Enumeration of the Species of Garden Ferns, with a Treatise on their Cultivation. By JOHN SMITH, A.L.S. New and Enlarged Edition. London: Hardwicke and Bogue.

No class of plants, probably, is for the present in such general favour as the ferns, and a work like the one before us must consequently find appreciative readers outside the circle of professed botanists. The author is one who has laboured long and zealously in the promotion of scientific horticulture. From 1822 down to 1864 he was curator of the Royal Botanical Gardens at Kew, where he devoted especial attention to this interesting group, and where the number of cultivated species was largely increased by his exertions.

In this Mr. Smith gives a history of the introduction of exotic ferns; an explanation of the terms used in describing them; an account of the principles on which they are classified, with the generic characters and an enumeration of cultivated species. The cultivation of ferns—whether in pots, in the open ground, in ferneries suited for tropical or sub-tropical species, or in Wardian cases—is described at some length, and in a manner which will be decidedly useful to amateurs, now so numerous and so zealous. Here, however, we cannot refrain from quoting with approval a remark of the author's on the present fern-mania:—"However laudable and agreeable fern-growing may be, yet it is to be regretted that it leads to the extinction of some of our rarest native species. Even the more common are becoming scarce in localities within easy reach, great quantities being yearly consigned to the London market." The misfortune is that many fern cultivators allow their pets to die yearly, and purchase a fresh stock on the approach of spring. The extinction of some species in certain localities we have ourselves observed and regretted. Thus we know of several dells in which *Polypodium dryopteris* formerly grew in some abundance, but

where it might now be sought in vain. Perhaps, however, the decrease of our choicer native ferns may be due not so much to the rapacity of cultivators as to the increasing pollution of the atmosphere in many parts of the country. Air fraught with coal-smoke is very uncongenial to many ferns and mosses, and checks their fructification.

In recommending this book to all who are interested in the study and in the cultivation of ferns, we must beg to express our sympathy with the author, who has been deprived of that sense which to all observers of Nature is dearer than life itself.

Narrative of the North Polar Expedition. U.S. Ship Polaris, *Capt. C. F. Hall commanding.* Edited by Rear-Admiral C. H. Davis, U.S.N. Washington: Government Printing-Office.

Polar exploration excites in men of different tastes and pursuits a widely different amount of interest. The biologist looks on almost with indifference, well knowing that a year's labour in the far north would yield fewer results of value than might be met with in a single day in climates more genial. The geologist and mineralogist, though well aware that important portions of the "great stone book" have been, in all probability, deposited around the poles, still fear that accumulations of ice and snow may for ever hide them from human perusal. On the other hand, the geographer, the meteorologist, and the student of terrestrial physics turn their gaze northwards with eager curiosity, and will doubtless never desist from their endeavours to solve the mystery of the poles till their efforts are crowned with success, or till many future adventurers, like Franklin and Hall, have found a glorious death. Nor can the risk and the possible sacrifice be considered to outweigh the prize that remains to be won. Material benefit, indeed, is out of the question. No one now explores the icy regions in the hope of finding an available passage from Europe, or from the eastern shores of North America to the Pacific; nor is the prospect of mineral wealth a temptation, for though valuable ores doubtless lie hid in these regions, the cost and difficulty of bringing them to daylight and transporting them to a market would be scarcely surmountable. But from a speculative point of view the inducements to polar investigations are tempting indeed. Setting aside the extravagant dreams of earlier days, such as the notion of an aperture leading down to beautiful and habitable regions in the interior of our globe, it remains to be decided whether the poles are covered, according to the theories of Adhémar and others, with massive caps of ice miles in thickness; whether they are, as some suppose, covered by open sea, enjoying a milder climate than the

regions immediately around them; or whether they are utterly undistinguished by any physical feature from the arctic wastes already explored. At present we can only infer, with more or less probability, as to what may be the condition of the earth at the extremity of its axis, and with this uncertainty we cannot feel satisfied.

Among the many who have sought to solve the "mystery of the Pole" none can claim a higher place than Capt. Charles Francis Hall, who, if merit could command success, might well have hoped to plant the flag of his country on the most northern part of the globe. He belonged to a class of men for whom we entertain a particular admiration. Like all true specialists, he had devoted his whole being to the solution of one question. For this he had thought, and studied, and prepared himself in every possible way, so that when the crowning opportunity came he might not be found wanting. To this end he sought to stir up public opinion in favour of a polar expedition under national auspices. He lectured in various cities of the Union; he corresponded with official personages, and, in short, left no stone unturned towards the completion of his chosen task. This part of Capt. Hall's life, indeed, reminds us of the journeys of Columbus when he went from court to court soliciting the means to put his project into execution. In pure, self-forgetting devotion to a great idea these two explorers are singularly alike. The American, however, unlike the Genoese, had to deal with a Government which honours and appreciates Science.

The work before us—whilst it recounts the progress of the Expedition, the illness and the lamented death of Capt. Hall, and the heroic struggles of the party after his end—does not detail the scientific results of the Expedition. The various journals and reports in which the observations made by the Expedition lie scattered have not yet been completely digested. Some of the documents, as well as of the specimens collected, appear to have been lost when the *Polaris* had to be abandoned.

Many interesting facts, however, are recorded in these pages. Those who imagine that mosquitoes are an exclusive feature of tropical climates will perhaps be surprised to learn that these pests, along with other two-winged flies, haunt even the far north. Two caterpillars were also found among moss, but of what kind it is not stated. The Mammalia of the region explored consist chiefly of musk-oxen, foxes, seals, lemmings, bears, wolves, and walruses. The Expedition therefore, though it approached the boundaries of animal life, did not actually cross them, as Captain Nares and his followers seem to have done. The fauna and the flora of the two Expeditions, when published, will be welcome as a contribution to our knowledge of the geographical distribution of organic life.

A curious physical fact recorded is the freezing of kerosene oil, which hydrocarbon at $-44°$ F. assumed a consistence like that of

melting wax, and a milk-white colour. No appearance of crystallisation was detected, even under the microscope.

There is one lesson to be drawn from this book which should on no account be overlooked. Copies of the "Narrative" have been forwarded, gratuitously and unsolicited, to universities, to learned societies, and public libraries, as well as to scientific journalists like ourselves, over the whole civilised world. By such acts of munificence the Government of the United States sets an example to its neighbours which we are sorry to see finds but very scanty imitation. Will England ever learn to "Americanise her institutions" in this respect?

Scepticism in Geology, and the Reasons for it. By VERIFIER. London: John Murray.

To this book a preliminary objection may be taken, serious, if not absolutely fatal. The author's ostensible position, as declared in his title and in his preface, is that of the sceptic—using the word of course in its philosophical and only legitimate sense. He selects as his motto the "*Ama nesciri*" of Thomas à Kempis. He calls for more thorough demonstration than geologists have in his opinion been able to furnish hitherto in support of their theories. Far be it from us to question his right to assume such an attitude, provided it be done really and consistently. Like every other science, Geology must be prepared to submit her conclusions to full and impartial criticism. She can claim our assent merely on condition of producing valid evidence. But what is scepticism? It is the dismissal of all preconceived notions bearing upon the question at issue; the refusal to accept without proof any proposition whatever, joined to complete indifference what conclusion may be arrived at provided it be in accordance with the logical interpretation of established facts. To give a familiar illustration, it is the frame of mind which the judge often explicitly enjoins the jurymen in cases which have excited a considerable amount of public feeling. They are told to dismiss all prejudices either for or against the prisoner, and utterly to ignore everything save the evidence which has been presented to them in court. This is true scepticism. But there is a pseudo-scepticism, very common among the opponents of what are called "modern scientific theories," which is combined with the grossest credulity, and which is in fact merely a consequence of the latter feeling. John Nokes doubts very strongly that $a = b$, and considers that in calling for additional evidence he is assuming the position of a sceptic. But what if all his doubts spring merely from a strong preconceived notion—not, let us bear in mind, resting upon any evidence—that a is not equal to b? He is then no sceptic, but a dogmatist in disguise. Such, we are sorry to

find is the real position of "Verifier." His mind, in approaching the question, is not "tabula rasa," but is filled with prepossessions and with an anti-geological—we can scarcely help saying utterly unscientific—dogmatism, which the work before us is an attempt to justify. "The assumptions of modern geology," he tells us, "have filled some minds with alarm." But alarm, or indeed emotion of any kind, is utterly to be avoided by the man who lays claim to the position of a sceptic.

It may seem that we have brought grave charges against the unknown author, but on a careful examination of the book they will appear only too fully justified. He seems to anticipate "rough usage," if noticed at all, possibly from some internal misgivings concerning his "facts" and his "arguments," but, as is common with writers of his school, he appeals to the "candid reader."

Our first proof that he is by no means the disinterested doubter, the modest enquirer he would fain appear, may be found in his remarks on geological time. He tells us "it is necessary to protest against the insatiable demands of geologists for time." He sneers at the observation of Darwin that it cost only 300 millions of years to denude the Weald. He declares there cannot be a more "groundless assumption" than the dictum of Mr. Geikie that "time is power." Can he pretend to misunderstand Mr. Geikie's meaning? "Would it not," he asks, "become the geologists of our time to abandon a position which enforces on their followers a belief almost amounting to a superstition?" Now we defy him to show any inherent or intrinsic improbability in the "untold ages," the "incalculable periods of time" which move his indignation. *A priori*, it is equally likely that the world has existed for six thousand, six million, or six billion years, and those who suppose the latter are not more credulous than those, if any, who still believe the latter. The advocates of Usher's chronology, who dream that "the world was created in autumn, 4004 years before the vulgar Christian era," have certainly no reason to tax geologists with credulity or superstition. Our argument in favour of the long past duration of the earth is drawn from the impossibility of compressing into a shorter time the occurrences of which the "stone book" has preserved the record. His grounds against the "incalculable ages" are never put forward at all, and may therefore, without any want of charity, be set down as a mere prejudice. It is instructive to note how eagerly, in default of any facts to prove the recent origin of the earth, he catches at shadows. He tells us in his Preface—"Darwin, Lyell, and others who proclaim a term of 300 millions of years insufficient for some of the operations of geology, are warned—'So much the worse for geology, since physical conditions render it impossible to allow her more than 10 or 15 millions of years.'" But does he not know that we regard these "physical considerations"—the recently attempted mathematical investigations into the maximum

age of the earth—as scarcely worth the paper upon which they are written? Mathematical demonstration is irresistible when applied to abstractions, but when it seeks to deal with realities it is often vitiated by being based upon false, or at best groundless, premises. The question is quietly and adroitly begged in such expressions as "Let us suppose," "If we only assume," &c., and the public—dazzled and awed by the display of formulæ—never look beneath at the flimsy foundation.

But the lingering belief in the recent origin of our earth is not his only prepossession. Towards the end of the work he declares his belief that the earth has not reached its present condition, but has been finished off at once according to specification, like a contract job—if we may be allowed the comparison—by a First Cause, acting directly and from without! Because the "scientific mind"—which his own is apparently not—repudiates an assumption so degrading, so unworthy of Deity, it is charged with "a stubborn resistance to the belief in a FIRST CAUSE." Can he fail to see that if such be the origin and the earlier history of the earth, the very existence of geology is destroyed? But let us quote some of the passages in which he places his anti-scientific creed in the fullest daylight:—"One vast fallacy would appear to underlie the doctrine of Modern Causes, the supposition that the world we inhabit—so beautiful, so pregnant with every gift which can contribute to man's progress, prosperity, and happiness—has been turned out by its Maker unfinished and imperfect; that it is capable of improvement, at least of development, and is undergoing material change day by day." "Is there any inconsistency in supposing that when a potter moulds a vase out of a lump of clay he should put forth his greatest energy and exert his utmost skill to finish it and turn it out perfect? That the work of the creation of the earth was one of perfection defies all disproof. What need, then, to imagine that it was done by little and little? least of all can we admit a solution of the problem of cosmogony, involving the absurdity that the work was left unfinished and needs constant alteration by means of certain mechanical self-acting operations. We venture to hope that the more geology is studied in an earnest spirit, free from the mirage of attractive but shadowy hypotheses, the more it will be acknowledged that not blind force, however gentle, nor mechanical impulses, however gradual, rendered our planet what we find it. It will eventually be acknowledged that at the time and in the process of fashioning the globe, a power was exerted totally different from the present course of nature." Here, then, the secret is revealed. The author's real difficulties in the way of accepting the truths of geology are his preconceptions of what is an "absurdity," of what is "perfection," and of how, in his opinion, the "First Cause" ought to have worked! With a man who can seriously maintain that the channels of rivers were made for them and not by them, it is impossible to argue. Had he been

a careful and sober-minded inquirer he could not have failed to find the superabundant refutation of what he calls his "arguments" and his "suggestions." He cannot see that "because a puddle in a rainstorm will cut runlets in the soft sand in an hour or two, it must follow that rain or running water will cut glens and valleys, or even sever high mountains, granted millions of years." Minds less filled with prejudices find in this no difficulty. The author ignores the fact that geologists only take up the history of the earth when no longer a nebular or an igneous mass. Consequently when Prof. Huxley speaks of a very remote period when the earth was passing through physical and chemical conditions which it can no more see again than a man can recall his infancy," he does not of necessity contradict Lyell when the latter declares that "the forces now operating upon earth are the same in kind and in degree as those which in the remotest times produced geological (but not pre-geological) changes."

"Verifier" asks—"If Nature were still carrying on operations by which the globe was made and fashioned, only on a diminished scale, are we not in the first place entitled to expect to catch her in the act of producing some of those *elementary* substances which enter into the composition of the earth's crust; not of depositing but of creating the metals and simple minerals, gold, silver, tin, quicksilver, iron, the diamond, emerald, &c.? In no instance has any such discovery been made." This is obviously an adaptation of the well-known anti-Darwinian argument, that no one has ever seen an animal or plant in the act of evolution. This objection is obviously of very little value, but as applied by "Verifier" it is even more unreasonable than in its original use. The transformations of organic beings might naturally be expected to take place on the surface of the earth, and if of a nature to attract attention might conceivably be noticed by man. But if *elementary* substances are being now created there is a very strong probability that the operation would take place underground, where no human eye could possibly witness. Again, is it not probable that all the materials of which our reputed elements consist have been used up in the production of such elements? It will be quite time enough to ask why we never witness the creation of gold when our chemists have detected a something out of which it could be created. Further still, supposing that tin or iron were in some place being actually created and not deposited, how could we assure ourselves that such was the fact? Suppose "Verifier" is standing by the side of a rock and sees a deposit of any metal forming upon its surface, and that with sufficient speed to be noticed, is he aware of the precautions necessary to prove that this is not a mere case of deposition? He tells us that "the sand and mud washed down into the Mediterranean by the Rhone in the days of Hannibal remain to this day incoherent sand and mud." How does he know this? and, painful as the question may sound to him, what is it to the purpose if

so short a time of exposure to the action of the sea should produce no decided action? We must therefore utterly repudiate his "obvious conclusion" that the conditions under which these substances were formed exist no longer.

"Verifier" next takes his stand upon metamorphism, by which he says, "sandstone may have been converted into quartz-rock, or even into granite." He ought to be aware that sandstone or quartz alone can never form granite, as two other constituents are requisite. To some, he tells us, "hot water charged with chemical carbonates seems to have been the agent" of metamorphism. Why some carbonates should be designated as "chemical" we are not sufficiently versed in pseudo-scepticism to comprehend. He asserts that "it has been impossible to realise metamorphism, or even to imitate it in the laboratory," and imagines he has reached "one unmistakable conclusion, viz., that metamorphism is a thing of the past, its processes not now discoverable, and it must therefore be dismissed from the category of 'Causes now in action.'"

If, however, the author had taken the trouble to make himself acquainted with the results of modern scientific research, he might have become a wiser and a less confident man. The researches of Messrs. Daubrée, Sterry Hunt, and others have decidedly proved that metamorphism can be imitated in the laboratory, and that neither a large quantity of water nor a very high temperature is requisite. We would recommend him to read the "Annales des Mines" (Ser. 3, vi., 78; Ser. 5, xii., 289; Ser. 6, vi., 78), "Memoires Academie des Sciences" (xvii., 1860), "Bulletin de la Soc. Geol. de France" (Ser. 2, iii., 547), "Ann. de Chimie" (xxiv., 258), "Quart. Journal Geol. Soc. of London" (xv., 488). His assertion that metamorphism is not still going on is utterly unproven, and it is, to say the least, contrary to all probability. There are certainly metamorphitic rocks which have been produced at comparatively recent dates. From the very nature of the case we cannot expect to "assist" —in the French sense of the word—at the process. Why metamorph products should be more common among the older than the more recent formations must be self-evident to every geologist.

Turning to another part of the book, we find the assertion that earthquakes are most common in plains and low grounds. This is exactly the reverse of the truth: the great earthquake regions—the western coast of South America, the West Indies, New Zealand, Iceland, Japan, the Malay Islands (excluding Borneo, which is more level), Calabria, the Iberian Peninsula, and the Greek Islands—are all mountainous. On the other hand, the vast plains of Eastern South America, of Central and Eastern Europe, and Northern Asia, are almost free from earthquake action.

There can surely be no occasion for us to wade further through

a book in which the plainest facts are denied or misinterpreted, and in which the all-important evidence of palæontology on the former elevations and subsidences of lands, and on their alternating connections and disconnections, is left out of view. A real "Scepticism in Geology" would be welcome, and would do the world good service, but it must produce very different evidence and be written in a totally different spirit from the book before us. "Verifier" has been nine centuries too late.

A Treatise on the Law Relating to the Pollution and Obstruction of Water-courses, together with a Brief Summary of the Various Sources of Rivers Pollution. By CLEMENT HIGGINS, M.A., F.C.S.; Barrister-at-Law. London: Stevens and Haynes.

IT is only under very exceptional circumstances that we can, with either benefit to our readers or credit to ourselves, presume to give an opinion concerning the merits or the defects of a work on any branch of law. Even in a case like the present, where we may lay claim to some acquaintance with, and at any rate a deep interest in, the subject-matter of the laws expounded, we can only deal with what may be called collateral issues. We may be able to discuss the question as to what constitutes "pollution" in a river. We may show the impracticability of certain standards that have been laid down, and point out others more feasible and rational. But the interpretation of statutes we must dismiss as a matter not within our competence.

The treatise before us is divided into two parts. The first of these is devoted to an exposition of the "Rivers' Pollution Prevention Act" of 1876. The author examines what constitutes an offence under the Act as to solid matters, sewage pollution, manufacturing refuse, and mining pollutions, and then treats of the institution of proceedings, and of facilities for conveying the refuse of factories into sewers. It is in this part of the work only that certain chemical questions arise. What constitutes "pollution" is the first difficulty. The author remarks that "The successful working of the Act will much depend upon the meaning placed upon the word "polluting" as therein used by those with whom its interpretation rests. It may be comparatively easy to determine whether any particular manufacturing, mining, or other refuse is poisonous or noxious, but it is certainly far more different, on account of the different meanings placed upon the word, to say whether it is or is not polluting. No attempt has been made by the framers of the Act to define the word, but they are content to say merely that the term "polluting" shall not include innocuous discolouration."

This exception seems to us an element of danger. No one

can contend that the colouring matters of logwood, fustic, peachwood, &c., if present in a river are likely to be injurious to man, beast, or plant. But who knows what accompanying and really noxious pollutions they may hide? On this account we think that transparency and the absence of colouration are characteristics to be desired, especially as they may be attained by known methods.

The author continues:—"Neither do the decisions of the judges either in courts of law or equity furnish a definition of the word such as to meet the requirements of the Act." As the only way of solving the difficulty he gives a summary of the expressed opinion of scientific men. Under this head we find once more the recommendations of the Rivers' Pollution Commissioners, on which surely nothing further need be said, especially as they have not been deemed fit for legislative adoption. Mr. Higgins gives along with them some—though by no means all—of the objections raised against them. He then quotes with marked approval the two recommendations given by Mr. Crookes, F.R.S., in his evidence before the Committee of the House of Lords. These are simply that the river itself should be the standard of purity, and that no liquid should be permitted to fall into a river if such liquid contains a greater percentage of impurity than the river itself. These suggestions, Mr. Higgins thinks, may be safely followed by county court judges in their decisions under the Act.

As regards solid matter thrown into rivers another difficulty arises. "No offence against the Act is committed unless the solid refuse, rubbish, cinders, or other waste or putrid solid matter finding its way into the river results either in *interfering with its due flow* or in *polluting its waters*." Here, we fear, the Act is scarcely stringent enough. The gradual silting up of rivers from natural causes is a source of danger which requires to be jealously watched and contended against, as the records of the past winter will show but too plainly. Surely, then, it is intolerable that the evil should be intensified by the sloth or the greed of those who would make the rivers general depositories of filth and refuse of all kinds. On this subject the Rivers Pollution Commissioners in 1868 spoke very judiciously. They recommended that not merely "the casting of solid matter of whatever kind into rivers and running water" should be prohibited, but also the placing of solid refuse in such positions on the banks of rivers as to render it liable to be washed away by floods." They also suggested that any Act passed for this purpose should take effect immediately.

In treating of sewage and of liquid manufacturing refuse, Mr. Higgins, though himself a chemist, and far better qualified to speak on the subject than many who have come forward as the instructors of the public, does not enter into the comparative value or efficiency of the various methods of dealing with polluted waters.

He even reminds manufacturers, municipal and sanitary authorities, &c., that "it is not the business of the Courts to give instructions in sewage purification.

The second part of the work is devoted to "Riparian Rights and their Protection." The author treats of such rights arising *ex jure naturæ*, or by way of easements or custom, of their protection by injunction or by an action for damages. The appendix contains statutory provisions relative to water-courses and the vesting of sewers. There are also tables of statutes and of cases.

As far as we are capable of judging this work is a valuable contribution to the literature of the sewage question, the law of which, as well as its chemistry and its engineering, requires to be brought clearly before the public.

The New Formula for Mean Velocity of Discharge of Rivers and Canals. By W. R. KUTTER. Translated from articles in the "Cultur-Ingénieur," by LOUIS D'A. JACKSON, A.I.C.E. London and New York: E. and F. N. Spon.

THE author tells us in his preface that "while the lead in engineering progress generally, both theoretical and practical, seems to have been almost entirely taken by the English-speaking races, and whilst improved construction, perfected appliances, and higher economy have progressed in the last thirty years at a speed perhaps greater than has ever been previously known, yet in the hydraulic branches of engineering no similar claim can be very satisfactorily made out for our own country. This seems at variance with our present requirements.

"We have in India a vast empire existing in a state of mutual dependence with England, whose enormous wealth is dependent on its population, whose population is dependent upon agriculture, and whose agriculture depends chiefly upon irrigation; where water is like silver, and the science of its judicious application and control is like gold. We have in semi-tropical regions large colonies which suffer from devastating floods alternating with drought." Thus far we are with the author; the evils which he has enumerated might doubtless be obviated by a proper system of reservoirs to catch the rains when they fall; of by-washes to carry the surplus part through towns and cultivated lands without occasioning devastation, and of conduits for the judicious distribution of the stored-up water in the dry season. To do these things is the work of the hydraulic engineer, and if he is to do them well he must not be required to work upon erroneous data. Two incidents in our modern domestic history —the Holmfirth and the Sheffield floods—confirm the author's view that hydraulic science—at least in its practical applications —has not progressed amongst us as might be desired. Is it

creditable to us that work done in præ-historical times of Ceylonese engineers should be better than some completed in England in the nineteenth century?

But Mr. Jackson goes on to say:—"At home the catchment areas of our rivers, in fact the country generally, is in a polluted state, the drainage both from farm lands and townships being still either badly regulated or under no general control. In spite of the increasing exceptions the water supply of most of our towns is so contaminated as to conduce, among other evils, to a fearful amount of intemperance, and the sewage, the natural regenerator of soils and crops, is generally allowed to mingle with noxious refuse, or to be so ill-regulated as regards dilution and application to land, that it not only ceases to be useful, but becomes a source of perpetual pollution." Here, we fear, our author is losing his way. We are not aware that intemperance is less rampant in Liverpool, Manchester, Glasgow, Leeds, Halifax, Huddersfield, Sheffield, and other towns supplied with uncontaminated waters from the mountains, than in such as draw their water from questionable sources. The selection of water and the disposal of sewage are, in our humble opinion, chemical not hydraulic questions. Further, defective as may be our water supply and our systems of drainage, we have still the satisfaction of knowing that in such points we are certainly not in arrear of our continental neighbours. The sanitary shortcomings to which we must plead guilty are due more to an over-tender regard for "vested interests" than to defective knowledge.

The text of the work before us is devoted to the flow of water in open channels generally, and secondly to the flow in open channels in earth. Then follow tables of the coefficients of mean velocity of discharge, of discharges and mean velocities per second, and a supplementary table of percentages for certain seconds. The work is a most valuable addition to English engineering literature, and we can only regret that it did not appear at an earlier date.

A Treatise on the Trisection of an Angle of Thirty Degrees and of any other Plane Angle. By BERNARD TINDAL BOSANQUET. London: Effingham Wilson.

THE author of this little pamphlet tells us that "with regard to his theory for the trisection of an angle of thirty degrees, he feels bound to admit that he has grave doubts as to its truth, having failed altogether in finding a geometrical solution." A fair examination of the question would be impossible without the aid of the author's diagrams.

SCIENTIFIC NOTES.

THE Meeting of the British Association for the Advancement of Science for 1877 was held at Plymouth, under the Presidency of Prof. Allen Thomson, M.D., LL.D., F.R.S. The attendance at the meeting was below the average, only 1217 tickets being issued. Several papers of great scientific interest were, however, read: we may mention in particular Sir William Thomson's paper on "The Possibility of Life on a Meteoric Stone falling on the Earth," and Mr. Preece's paper on "The Telephone," both read in Section A. Great amusement was caused by Sir William Thomson saying that, though the outside shell of a meteoric stone might be incandescent from the friction caused by its flight through the terrestrial atmosphere, yet within a crevice of that stone might be concealed a Colorado beetle, which, falling on the earth, might become the father of a large and prosperous family. The lecture to working men was delivered by Mr. Preece, the subject being "Telegraphy." At the conclusion of the lecture the telephone was connected with the Plymouth and Exeter Post-offices, and ordinary telegraphic operations were suspended, in order that the respective places might be communicated with through the telephone. The experiments were conducted by the President, Mr. Preece, and Sir William Thomson, the latter gentleman remarking that the sounds came more perfectly than he had ever heard through a speaking tube. At the final meeting of the Mechanical Science Section the telephone was also exhibited and described by the inventor, Mr. Graham Bell, who arrived from the United States during the meeting of the Association. Mr. Bell, after observing that many years ago his attention was directed to the vibrations occasioned in the air by vocal utterances, gave a history of the experimental researches which he had conducted with the view of discovering means by which the sound of the human voice could be successfully conveyed to whatever place was desired, and which had led to the invention of the telephone. He had four different kinds of instruments, and was uncertain which was the best; but experiments were being conducted at the present time for the purpose of making improvements and developments. The invention was therefore still in embryo. Mr. Bell then announced that he had brought with him his telephonic organ, which resembled a harmonium or parlour organ. The reeds were connected with a battery, and in front of each reed there was a little screw with a platinum point. When the instrument was blown the reeds vibrated against the screws, which were connected with a telegraph wire, and on contact being made the music was conveyed. The organ was at the Guildhall, and was connected by a telegraph wire with both the room and the Post-office, the battery being at the latter place. Mr. Preece then attached the telegraph wire to a telephone, and inquired if his assistant, Mr. Harris, was at the Post-office. The immediate reply was "Yes, sir." Mr. Preece then said, "Will you put the wire through to the Guildhall, and ask the player there to give us a tune?" "Right; I will do it at once," was the answer. After a few moments the strains of "God Save the Queen" were distinctly heard by the audience. Mr. Bell explained that the sounds had been produced by a voltaic battery, and the music should next be without the aid of a battery. The sounds would be much fainter, but might be audible to those who were near the platform. The "National Anthem" was again played, but less distinctly than before. At the request of the professor Mr. Harris sang a song through the telephone from the Post-office, and "Auld Lang Syne" was heard with great clearness.

At the end of the Plymouth Meeting of the British Association, Sir William Thomson alluded to the great loss the Association has sustained in the death

of Mr. Gassiot, F.R.S. He alluded to the interesting communications of experiments made by Mr. Gassiot in Section A at Brighton, and to the enormous harvest of results that had come into it as the result of his munificence. It was through Mr. Gassiot's foundation of £500 a year that the maintenance of the Kew Observatory was ensured. That was a foundation in perpetuity, and the results already derived from it were of such obvious importance in connection with that Section that it was right they should consider them now the founder was no more. Mr. Gassiot's life had been one of usefulness—usefulness in business—among his friends; and, above all, in the wider scientific sphere in which the later years of his life had been employed. That he should have lived so long—over 80 years—and won so much good feeling, respect, and admiration, was enough to make him, whether in life or in death, to be equally envied.

A Special Meeting of the California Academy of Sciences was held on August 30th, for the purpose of extending a formal welcome to Sir Joseph Hooker, C.B., Dr. Asa Gray, and Prof. F. V. Hayden. The chair was taken by Prof. Davidson, the President of the Society. In returning thanks for the cordial welcome given, Sir Joseph Hooker said:—"The President has asked me to say a few words with respect to the Academy. In England we know well enough what it is to wait for results; but the destinies of Science on this coast are great, and a time will come that will show great results, and that will come with immense force, and for these two reasons:—There is here a most intelligent and a most active and progressive population, and, in the second place, there is here one of the most remarkable assemblages of natural objects and physical phenomena that any part of the world possesses. In speaking thus I include the whole coast north and south of California. There is no section of the earth in which so many singular phenomena can be observed as in this. Without seeking to give advice I may point out what has been the element of success in the greatest Academy of England, the Royal Society. It began with very few men, and for the best part of two centuries it was supported by what he might, without disrespect to his ancestors in science, call elderly people. It was by the elderly men who loved Science, holding together congenially year after year, and almost century after century, that the young men of the Society were drawn to it, and it is but lately that young men in any numbers have come into the Society. For success there are three principal elements—the holding together of the elderly members, of those who have had experience of this life in other matters than Science, and who bring that experience together, with methodised common sense, of which Science consists, to bear upon the objects of the Society itself. In the second place, there is the important work of the Secretary, together with that of the Publication Committee, which should carefully pass judgment upon the communications to be given to the world. The supervision of the papers of a Society by several members is perhaps the most important scientific work that any Society can perform. Thirdly, there is the necessity of looking well after the funds, and managing them with economy and prudence." Prof. Hayden, in responding to the welcome, indicated the features of the geological survey in progress under his direction, and said he has long desired to make some comparison between the Sierra Nevada Mountains and the Rocky Mountains. It had always been his belief, although the belief had been corrected by his studies of the Eastern Slope, that there is a general geographical as well as geological unity in all the different ranges of mountains that compose our country. Other geologists have endeavoured to give to the Sierra the name of the Cordilleras, as a generic term, extending it to the Andes and to the Eastern Range, the Rocky Mountains. Other geologists have sought to make the Rocky Mountains the generic name, including in that range all the rest, and making the Sierra Nevada a branch. He was now inclined to think there is difference enough in the two ranges to regard them as separate, and perhaps almost independent ranges. One object of his visit was to examine the Yosemite Valley, and study the phenomena of its formation, and this he had been enabled to do. At some other time he hoped to be in a position to study the geology of the coast carefully.

GEOLOGY, PALÆONTOLOGY, &c.—Dr. F. V. Hayden has issued further valuable Reports on the progress of the U.S. Geological Survey of the Territories. The "Preliminary Report of the Survey of Montana and Portions of Adjacent Territories, being a Fifth Annual Report of Progress," contains, among its more important features, Dr. Peale's report on the minerals, rocks, and thermal springs of the region; Prof. Thomas's remarks on its agricultural resources; palæontological notices, by Messrs. Lesquereux and Cope; and an account of the present zoology and botany of the territory, by Messrs. Leidy, Horn, Uhler, and Porter. Turning first to the last-mentioned subject, we find it recorded by Dr. Horn that, in the Coleoptera at least, the higher the elevation and the colder the climate the more deeply sculptured is the species. "Species appear to be distributed on lines of country presenting as nearly as possible similar meteorological conditions. Thus many Oregon forms extend southward into California, gradually seeking a higher mountain habitat as the region becomes warmer." The catalogue of Coleoptera will prove suggestive to all who are interested in zoological geography. As regards the classification adopted we cannot but protest against the group "Scarabæidæ," made to include all the Lamellicornes except the Lucanidæ, and forming thus a division of a very different rank from the "Scarabæidæ" of MacLeay. The well-known potato-beetle, *Doryphora decemlineata*, is indeed mentioned in the catalogue, but the curious fact is noticed that its progress is not westwards. The Hemiptera of the territory are not merely catalogued, but described systematically. The Orthoptera, which here include many species formidable to the agriculturist, are also described at length. The palæontological report is exceedingly interesting. Among the conclusions drawn from the fossil botany of the region is that the "Tertiary flora of North America is by its types intimately related to the Cretaceous flora of the same region. All the essential types of our present arborescent flora are already marked in the Cretaceous of our continent, and become more distinct and more numerous in the Tertiary; therefore the origin of our present flora is, like its *facies*, truly North American. The relation of the North American Tertiary flora with that of the same formation of Europe is marked out for North American types, but does not exist at all for those which are not represented in the living flora of this continent; therefore the European Tertiary flora partly originates from North American types, either directly from this continent or from the Arctic regions. The species of plants common to the Cretaceous and Tertiary formations of the Arctic regions and of our continent indicate in the mean temperature influencing geographical distribution of vegetation a difference in +, equal to about 5° of latitude for the Tertiary and Cretaceous epochs." The paper on the geology and palæontology of the Cretaceous strata of Kansas opens with a forcibly-written general sketch of the ancient life. In the remains of huge reptiles the district is exceedingly rich. "If the explorer searches the bottom of the rain-washes and ravines he will doubtless come upon the fragment of a tooth or jaw, and will generally find a line of such pieces leading to an elevated position on the bank or bluff where lies the skeleton of some monster of the ancient sea. He may find the vertebral column running far into the limestone that locks him in his last prison, or a paddle extended on the slope as though entreating aid; or a pair of jaws lined with horrid teeth, which grin despair on enemies they are helpless to resist." Twenty-four species of fossil reptiles have been found in Kansas, varying from 10 to 80 feet in length, and representing six orders. Two only were terrestrial, two were winged, and the remainder were oceanic. The rulers of the waters of ancient America were the Pythonomorphæ—creatures much resembling monstrous serpents, but furnished with two pairs of paddles like the flippers of a whale. One of the species, *Liodon dyspelor*, appears to have reached the length of 75 feet. Among the flying saurians the most formidable was *Ornithocheirus umbrosus*, which measured nearly 25 feet across its wings. Among the illustrations will be found a map of the great national Yellowstone Park, and plans, and views of the geysers which appear to surpass those of Iceland.

Under the title of "Preliminary Report of the United States Geological Survey of Wyoming and Portions of Contiguous Territories," we find a general review of the geology of the country from Omaha to Salt Lake Valley. Now that catastrophism is again attempting to raise its head, and now that outsiders are hugging themselves with the notion that the "guns of geology are being turned upon biology," one passage in this review becomes doubly important. We may parenthetically point out the inconsistency of the "rump" of the Cuvierian school, who one moment declare that Evolution must be a myth because certain species still surviving can be traced back to the earlier geological epochs, but the next pronounce it baseless because from time to time every living being has been swept away by some violent and general convulsion! The writer of the work before us remarks:—"We have already obliterated the chasm between the Permian and the Carboniferous era, and shown that there is a well-marked inosculation of organic forms, those of supposed permian affinities passing down into well-known carboniferous strata, and admitted carboniferous types passing up into the permian. We believe that the careful study of these transition-beds is destined to obliterate the chasm between the Cretaceous and Tertiary periods, and that there is a passing down into the Cretaceous period of tertiary forms, and an extending upwards into the Tertiary of those of cretaceous affinities. . . . If there is a strict uniformity in all the operations of Nature when taken in the aggregate, as I believe there is, then this is simply in accordance with the law of progress, which, in the case of the physical changes wrought out in the geological history of the world, has operated so slowly that infinite ages have been required to produce any perceptible change. The position I have taken in all my studies in the West is that all evidences of sudden and paroxysmal movements have been local, and are to be investigated as such, and have had no influence on the great extended movements which I have regarded as general, uniform, and slow, and the results of which have given to the West its present configuration." In a chapter on the fossil plants with reference to our present civilisation, the difference of origin between coal and petroleum is thus explained:—"Petroleum is, like coal, the result of slow maceration of plants; with this difference, that in the formations of the coal the plants which entered into the composition of the matter were woody or fibrous, and the woody tissue cannot be destroyed by the process of slow combustion any more than it is in burnt charcoal. The plants which concurred to the formation of petroleum were sea-weeds. These have no fibre, no wood in their tissue, which is merely cellular, and in their decomposition all trace of this tissue has been destroyed and pure bitumen preserved, either by impregnation of shale or sandstone or by accumulation in subterranean cavities." It is to be regretted that matter so interesting should be conveyed in a style so cumbrous and feeble. Near the junction of the White and Green Rivers, on the borders of Colorado and Utah, is an immense tertiary deposit, consisting of petroleum shales abounding in the impressions of leaves and of various insects:—"Between sixty and seventy species of insects were brought home, representing nearly all the different orders: about two-thirds of the species were flies, some of them the perfect insect, others the maggot-like larvæ; but in no instance did the imago and larva of the same insect occur. The greater part of the beetles were very small. There were three or four kinds of Homoptera, ants of two different genera, and a poorly preserved moth. Perhaps a minute *Thrips*, belonging to a group which has never been found fossil in any part of the world, is of the greatest interest. It is astonishing that an insect so delicate and so insignificant in size can be so perfectly preserved in these stones: the wings remain uninjured, and every hair of their microscopic fringe may be counted." Here, as in other parts of the world, we notice among the fossil Mammalia forms which combine the characteristics of what are now distinct species, or even genera, and are thus important evidence in favour of the doctrine of Evolution. From the Bridger Rocks Prof. Leidy reports the former existence of an animal possessing an affinity to the hyæna and the panther. It was larger than the couguar (the so-called "panther" of the United States), and was a predaceous animal of great strength, and doubtless of equal ferocity.

"It has been named *patriofelis ulta*, which signifies the ancestral cat that hath revenged itself." How it has avenged itself, or upon whom or wherefore, we do not learn. Still more remarkable is the *Hyænodon*, three species of which have been discovered in the Lower Miocene tertiaries of White River. In their anatomical character they approach at once the wolf, the tiger, the hyæna, the weasel, the racoon, and the opossum. *Hyænodon horridus*, three imperfect skulls of which have been found, was doubtless the most formidable carnivorous land animal of its time. Another of these missing links or intermediate forms is Dr. Leidy's "ruminant hog." Like the tame swine it was provided with incisors and canines, but its molars were constructed upon the pattern of living ruminants. They were four-toed, and unprovided with horns. The former development of the camel and of the horse tribe in America is most remarkable. The only camel-like animals at present existing in the western hemisphere are the llama and its congeners, in the mountainous parts of South America; yet in the valleys of the Niobrara and the Loup Fork are found the fossil remains of a number of species of extinct camels, one of which was of the size of the Arabian camel, a second about two-thirds as large, and a third smaller. In like manner no horses existed in America prior to its discovery by Columbus; yet Dr. Leidy reports twenty-seven species of *Equidæ* which lived on this continent prior to the advent of man, "about three times as many as are now found living throughout the world." Among the Pachydermata were a true hog as large as the African hippopotamus, five species of rhinoceros, a mastodon, and a large elephant not as yet discovered in any other part of the world. The author is inclined to infer, from the totality of the phenomena observed, that the Western continent is the original home of animal life, and instead of being the "New World" is older than the land in the Eastern hemisphere. The fossils found are remarkable for their beauty and perfection, and cannot have been transported from a distance or exposed to the action of turbulent waters, as they "seldom show any signs of having been water-worn, and the sharp points and angles are as perfect as in life." Turning from fossilised to living beings, we find an elaborate and interesting account of the *Caloptenus spretus*, or hateful grasshopper—a scourge in its native haunts, at least, more formidable even than the Colorado beetle, but fortunately less able to accommodate itself to differences of climate, elevation, and soil. Did space allow we might go on almost indefinitely multiplying interesting extracts from this volume. One defect in the getting up of the work we must beg to point out:—In many scientific books the heading to each page is a key to the subject-matter below. In others the heading of the right-hand page alone serves as a running table of contents, whilst that of the left-hand page merely repeats in brief the title of the book; but in the volume before us each page is merely headed "Geological Survey of the Territories," which makes it difficult to find any particular passage of which the reader is in quest.

No. 3 of vol. iii. of the "Bulletin of the United States Geological and Geographical Survey of the Territories" contains articles on the comparative vocabulary of the Utah dialects; on the method of making stone weapons; on a peculiar type of eruptive mountains in Colorado; notes on the geology of the region of the Judith river, Montana, and on vertebrate fossils found near the Missouri: with a series of palæontological papers referring chiefly to Unionidæ, Physidæ, and other Mollusca. Among the fossils from the Judith river are a number of the bones of a large Deinosaurian reptile which show not merely Avian but also Mammalian affinities, such as the great coronoid process of the dentary bone, which is a feature unknown among birds and reptiles.

The "Miscellaneous Publications" include "Lists of Elevations, principally in that portion of the United States West of the Mississippi River." The number of summits in the Rocky Mountains ranging from 12,000 to 14,000 feet above the sea-level is remarkable, but greater heights are rarely attained. The loftiest peaks are Mount St. Elias, in Alaska, variously stated at from 16,938 to 19,500 feet above the sea-level, and Mount Cook, also in Alaska, 16,000 feet.

The Director of the Geological Survey of Canada, A. R. C. Selwyn, F.R.S., F.G.S., has issued the "Report of Progress for 1875—1876." The Dominion, like its southern neighbour, the American Union, is sparing no pains in ascertaining the nature and the magnitude of its natural resources. For this purpose exploring expeditions are being sent to report on the mineral wealth of the back territories, their agricultural capabilities, their climate, and the means they offer for communication. Nor are these investigations limited to the economic features of the country. Its geological characteristics, its fauna and flora, both living and extinct, are carefully scrutinised. That this conduct is true wisdom, even from a practical point of view, stands in no need of demonstration. The general impression which must be left on the mind of the reader is that the value of the western, and even the northern, parts of the Dominion is far greater than we in England commonly imagine. Thus even in Fort Simpson—latitude 62° N.—barley always ripens between the 12th and 20th of August, and wheat succeeds in four seasons out of five, which is more than can be said of some localities in England. Vancouver Island and the Queen Charlotte group resemble in their climate the western shores of Ireland, but are warmer and less troubled with summer rains. A current analogous to the Gulf Stream, the "Kuro Siwo," is the cause of a temperature abnormally high for the latitude. The forest growth is magnificent, the soil fruitful, the deeper strata rich in gold, silver, coal, and iron. The coast-line of 400 miles in extent is indented with inlets—here called canals—resembling the fiords of Norway, and affording everywhere safe anchorage for ships. In short, it would be difficult to imagine a region better fitted by Nature to be the home of a great industrial and commercial people. The botanist, Mr. Macoum, speaks with enthusiasm of the beauty and luxuriance of the vegetation, of which he gives lists. This volume contains an obituary notice of Sir W. E. Logan, the first Director of the Geological Survey of Canada, to whom, indeed, the success of this undertaking is very largely owing. It is scarcely needful to remark that this Report, like its predecessors, constitutes an invaluable storehouse of facts for all who are interested either in the geology, the palæontology, or the botany of the Dominion. Its utility is enhanced by the accompanying maps and views of the scenery taken from photographs.

From Section XI. of Dr. Ottokar Feistmantel's "Memoirs of the Geological Survey of India—Palæontologia Indica, being Figures and Descriptions of the Organic Remains procured during the Progress of the Geological Survey of India," which treats of the Jurassic (oolitic) flora of Kach, we learn that the flora of Kach is very poor when compared with the other fossil floras of India, especially in the number of species. In age it probably approximates to the Lower Oolite of Yorkshire, as seen at Scarborough, with which it has eight forms identical. With the oolitic floras of France and Italy that of Kach has no species in common.

Microscopy.—"Double-Staining Tissues with Indigo-Carmine and Carmine," by F. Merbel ("American Journal of the Medical Sciences," for January, 1877). The dye consists of two fluids made separately, but mixed before use. One, a boracic solution of carmine (carmine, ½ drachm; borax, 2 drachms; distilled water, 4 ounces). The other, a similar solution of indigo-carmine (indigo-carmine, 2 drachms; borax, 2 drachms; distilled water, 4 ounces). Indigo-carmine is the trade name for sulphindigotate of potassium, and is the same dye used by Chrzonszczewsky in his researches upon the commencement of the portal duct. The specimens to be stained, if hardened in chromic acid, must be deprived of it by washing, and then be immersed in the mixed dyes for a quarter of an hour. After that they must be transferred to a saturated solution of oxalic acid, both to "set" the blue colour as well as to lighten the general tint, and then, having been washed in distilled water, mounted in Canada balsam in the usual way. The author does not consider the process as yet perfected, but hoped that, by finding some other reagent than oxalic acid, that would possess its good without its deleterious properties, a more uniform and certain result might in all cases be ensured.

The Rev. A. Renard, of Louvain, has contributed to the "Transactions of the Royal Microscopical Society" a valuable treatise on the "Composition and Microscopical Structure of the Belgian Whetstones." The rocks examined were those in the neighbourhood of Viel-Salm, in the province of Liege, Belgium. The stones are in shape parallelopipeds, composed of a stratum more or less deeply coloured yellow, and of a stratum coloured blue-violet, and constitute the well-known "razor-hone." The hones of Viel-Salm are found amongst the rocks which Dumont calls Salmien, but seem to resemble very much the English Cambrian, and to be the equivalent of the schist of Tremadoc. The blue and yellow layers, although apparently separated by a distinct straight line, are so intimately united that the workman has nothing else to do than to square the fragments. The minute structure of the rock is very remarkable. A fibrous pliation oblique to the stratification is prolonged from the slate into the whetstone; and if the slate be broken the lamina comes off, passing across the whetstone and the fissure is prolonged with a regularity and constancy of angle which shows evidently that the two rocks have a common cleavage. The author is of opinion that the whetstones are contemporary layers with the incasing slates, and have been subjected to the same mechanical actions as all of the phylladic rocks of this group. The mineral composition and the microscopical and optical structure have been carefully studied by the Abbé Renard. The matter is highly interesting, but does not admit of a useful abstract being made, and occupies too much space to quote in full. The paper affords another instance of the great value of the microscope, and especially the service rendered by the polariscope to the investigation of substances hitherto regarded as almost structureless.

Specimens of fossil Polyzoa are frequently obtained in which the least interesting side is the one exposed to view. Mr. John Young, of the Hunterian Museum University, of Glasgow, has communicated to "Science Gossip" (July, 1877, p. 158) a process by which the specimens may be detached from the original matrix, and mounted so as to show the side hitherto out of view. The specimens suitable for the process are those imbedded in shales that yield readily to the disintegrating influence of the weather. Very little can be done with specimens imbedded in hard calcareous shells or limestone. After selecting the specimens of Polyzoa it is best to let them be well dried at a fire or in the sun for a few days, to secure the adhesion of the asphalte used in mounting. When a specimen is to be operated upon let it be heated before the fire, and a layer of asphalte melted over it with a piece of iron heated nearly to redness. The asphalte used is the common sort, free from sand, employed in street paving. While hot, press a piece of tough brown paper down over the surface evenly with the fingers. The paper strengthens the asphalte very much, and afterwards, when the specimen is mounted, the paper adheres to the tablet on which it is fastened better than when asphalte is used alone. When large fronds of Polyzoa have to be lifted from the shale, then a second layer of asphalte and brown paper is employed: this forms a firm thin cake, which in large specimens is less likely to break across. The next operation, after fixing the asphalte to the fronds of the Polyzoa, is to place the specimens in water, and let them lie until the shale softens. In some cases the Polyzoa parts from the shale in a few minutes; in others an hour or two, or even a day, may be required. The process may be hastened by placing the specimens in a saucer filled with water, and, as the shale is softened, keep picking it away with a thin sharp knife until the fronds of Polyzoa appear; then with a worn nail or tooth-brush mash the surface of the specimens until they are quite clean and the cell-pores well exposed. If the fronds have been well fixed to the asphalte, the greatest freedom may be used in mashing the specimens without fear of their removal by the brush. For cleaning small specimens of Polyzoa intended for microscopical examination, the following method may be employed:—After having picked the specimens out from amongst the weathered limestone shales, where they often have a thin layer of clay adhering to them, take a glass slide and cover it with a layer of thin gum; then with the forceps lift all the fragments of Polyzoa to be cleaned, and place them on the slide with the poriferous face uppermost, afterwards

allowing the slide to dry slowly for a day or two. When the gum is quite hard, place the slide in a saucer of water, and brush the specimens gently and quickly with a nail- or tooth-brush. The gum will hold the fragments of Polyzoa firmly and safely in position, quite long enough before dissolving, so as to allow the specimens to be well cleaned: when this is done allow the slide to lie in the water until all the specimens are dissolved off from the surface; they can afterwards be collected with a soft hair-pencil, and dried upon blotting-paper, when they are quite ready for mounting.

A new form of section-cutting machine for general microscopical purposes, by Mr. H. F. Hailes, is described in the "Transactions of the Quekett Microscopical Club" (July, p. 243), to which those interested are referred, as an abstract of the paper would be useless without the drawings of the instrument.

TECHNOLOGY.—The Clothworkers' Company of London having, with a wise and truly patriotic munificence, founded and endowed a Department of Textile Industry in connection with the Yorkshire College of Science at Leeds, Mr. J. Beaumont, the "instructor" to the Department, along with Mr. McLaren,—of the firm of Smith and McLaren, of Keighley,—were commissioned to visit the weaving and other technical schools of France, Belgium, and Germany. The result of their observations has been embodied in a Report, which is published by Rivingtons, and which we commend to the heedful attention of all interested in the manufactures of this country. The following passage, which, *mutatis mutandis*, applies to all branches of manufactures, is an excellent answer to a common objection. It is said by many that if a young man wants to learn weaving, let him go to a first-rate mill. "The answer is that the weaving school is not meant to supplant the training received in the mill, but to supplement it in that particular where the latter fails. The apprentice in a mill will see other men designing and arranging new patterns; but will he learn to design and arrange them himself, to calculate the warp and weft required to weave them, or to cut the cards or arrange the healds? In a well-managed factory, where everything goes as if by clockwork, no one has time to teach a learner these things. *Even if there were the time, there might not be the desire to teach.* The jealousy of overlookers is often so great that, instead of helping a person who comes to learn, they not unfrequently do much to hinder him." We can confirm this statement from our own observation in such branches of industry as have come under our notice. The very object of apprenticeship is, in these days, to create and uphold a monopoly of practical knowledge. The fee is received, and as little as possible is given in return. Full testimony is borne to the striking success of the Polytechnic School of Aix-la-Chapelle. "Though very large and complete, another building of equal size is being erected by its side, which is to form the chemical department." There are now five hundred students, with twenty-four professors and eighteen assistants. The following passage may supply food for reflection:—"We noticed a number of packing-cases which we were told contained models of English patent machines, sent as a present by the British Government at the request of the Prince Imperial of Germany. We would suggest that the Government should be invited to extend its liberality to the (Leeds) College of Science, and to similar institutions in their own country." We might ask whether such international courtesies should not be made dependent on reciprocity and on international patent-right? At present it seems rather hard upon the English inventor whose application for a patent in Germany has been in due course refused to have his models sent abroad, and therefore the more conveniently pirated.

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